THURSDAY, DECEMBER 28, 1893.

QUATERNIONS AS AN INSTRUMENT IN PHYSICAL RESEARCH.

Utility of Quaternions in Physics. By A. McAulay, M.A. (London: Macmillan and Co., 1893.)

JUST as "one shove of the bayonet" was truly said to be more effective than any number of learned discussions on the art of war:—this really practical work, giving genuine quaternion solutions of new problems as well as largely extended developments of old ones, is of incomparably greater interest and usefulness than the recently renewed, but necessarily futile, attempts to *prove* that a unit vector cannot possibly be a quadrantal versor:—nay, that a Calculus of Vectors must limit itself to the beggarly elements of addition and subtraction, commonly called "composition."

It is much to be regretted that Mr. McAulay has not determined simply to let his Essay speak for itself. His Preface, though extremely interesting as the perfervid outburst of an enthusiast, assumes here and there a character of undignified querulousness or of dark insinuation, which is not calculated to win sympathy. It has too much of the "Rends-toi, coquin" to make willing converts; and in some passages it runs a-muck at Institutions, Customs and Dignities. Nothing seems safe. It is a study in monochrome:—the lights dazzlingly vivid, and the shades dark as Erebus! We gladly pass rom it to the main contents of the book.

There can be no doubt whatever of its value from the scientific point of view. It is the work of a man of genuine power and originality. Many parts of it are, no doubt, laboured and somewhat heavy, others very crude; and in some places the obscurity is almost repulsive. [Curiously, these obscurities occur chiefly where more than usual pains have been taken to make things plain!] But faults like these are well-nigh inevitable in a first effort; and they should, perhaps, be regarded as enhancing by contrast the merits of the novel processes and results to which they act as a foil.

It is positively exhilarating to dip into the pages of a book like this after toiling through the arid wastes presented to us as wholesome pasture in the writings of Prof. Willard Gibbs, Dr. Oliver Heaviside, and others of a similar complexion. Here, at last, we exclaim, is a man who has caught the full spirit of the Quaternion system:—"the real *œstus*, the *awen* of the Welsh bards, the *divinus afflatus* that transports the poet beyond the limits of sublunary things"! No doubt, to a man like this, the restrictions imposed, in view of the prospective ordeal of the Senate-House, by the passionless worldly-wisdom of a "Coach," must have been gall and bitterness. Intuitively recognising its power, he snatches up the magnificent weapon which Hamilton tenders to all, and at once dashes off to the jungle on the quest of big game. Others, more cautious or perhaps more captious, meanwhile sit pondering gravely on the fancied imperfections of the arm; and endeavour to convince a bewildered public (if they cannot convince themselves) that, like the Highlander's musket, it requires to be treated to a brand-new stock, lock, and barrel, *of their own devising*, before it can be safely regarded as fit for service. "Non *his* juventas orta parentibus . . ." What could be looked for from the pupils of a School like that?

Mr. McAulay himself has introduced one or two rather startling innovations. But, unlike the would-be patchers or underpinners to whom we have referred, he retains intact all the exquisitely-designed Hamiltonian machinery, while sedulously oiling it, and here and there substituting a rolling for a sliding contact, or introducing a *lignum vitæ* bearing. To borrow an analogy from current electricity, he endeavours to add facilities, while his concurrents are busy adding resistances, sometimes indeed breaking the circuit altogether!

Among the additions to which Mr. McAulay calls attention, some are certainly not novel, they were perfectly well known to Hamilton himself. Thus the use of suffixes, to show which factor of a product (say) is to be acted on by an operator, is at least as old as Herschel's *Appendix* to the translation of Lacroix:—and is an essential part of the notation required for what is correctly called "Hamilton's Theorem." Mr. McAulay refers to this as a process of his own, which was found "necessary somewhat to expand the meaning" of a symbol. Another instance is the use of a vector, *which may have an infinite number of values*, for the purpose of condensing three independent scalar equations into one common expression, &c. This is purely and entirely Hamiltonian.

The "startling innovations," however, as we called them above, are unquestionably Mr. McAulay's own—and he has certainly gone far to justify their introduction. He has employed the sure tests of ready applicability and extreme utility, and these have been well borne. Objections based upon mere unwontedness or even awkwardness of appearance must of course yield when such important advantages as these (if they be otherwise unattainable) are secured; but it certainly requires a considerable mental wrench to accustom ourselves to the use of

$$X_1 \frac{d}{dx_1}$$

as an equivalent for the familiar expression

$$\frac{dX}{dx}.$$

If this be conceded, however, it is virtually *all* that Mr. McAulay demands of us, and we are free to adopt his system. It is to be carefully observed that there is no interference with the *principles* of quaternions to which, as was remarked above, Mr. McAulay strictly adheres. The quantities and operators, to which the dislocation applies, *are all scalars*, and the wrench referred to is therefore an algebraic, not a specially quaternion, one. Its introduction is made necessary by the determination to adhere to the non-commutative property of quaternion multiplication, while endeavouring to effect certain desirable transformations. Mr. McAulay likens this dislocation of the usual arrangement of operator and subject to the occasional disarrangement of relative position of adjective and substantive in a Latin sentence:—the nexus between them being the common case-ending, which is the analogue of the common suffix. A single example,

of a very simple character, must suffice. Thus in the strain of a homogeneous isotropic solid, due to external potential u, we have for the strain-function ϕ (when there are no molecular couples) the equation

$$S.\nabla\phi\alpha + S\alpha\nabla u = 0,$$

which (in virtue of the property of α, already spoken of) is equivalent to three independent scalar conditions. Suppose we wish to express these, without the α, in the form of one vector condition. Mr. McAulay boldly writes the first term as

$$S.\alpha\phi_1'\nabla_1 \text{ ; or rather as } S.\alpha\phi\nabla,$$

for in so simple a case the suffixes are not required, and the strain-function is self-conjugate under the restriction above. Then, at once, the property of α shows us that

$$\phi\nabla + \nabla u = 0,$$

which is the vector equation required. Here it is obvious that, in the usual order of writing,

$$\phi\nabla = \frac{d}{dx}\phi i + \frac{d}{dy}\phi j + \frac{d}{dz}\phi k.$$

This simple example shows the *nature* of the gain which Mr. McAulay's method secures. Those who wish to know its *extent* must read the work itself. They will soon be introduced to novel forms of concentrated operators with regard to which, as I have not yet formed a very definite opinion, I shall content myself by hazarding the remark that, while they are certainly powerful and eminently useful, they must at present be regarded as singularly uncouth.

As a purely personal matter I would add that I do not think Mr. McAulay states quite accurately the nature of some of the remarks which I made on his Essay when I read it (at the request of Dr. Ferrers) in MS. The passage of the last edition of my book, to which he refers as being aimed at him, was meant as a defence or explanation of my own procedure. So far as I can recollect, I urged Mr. McAulay to avoid (when possible) dealing with quaternion *elements;* and to frame relations between surface and volume integrals, &c., from kinematical or other interpretations of their *wholes*.

P. G. TAIT.

THE MANUFACTURE OF PAINTER'S COLOURS AND VARNISHES.

Painter's Colours, Oils, and Varnishes: a Practical Manual. By George H. Hurst, F.C.S. (London: C. Griffin and Co., 1892.)

THIS work is intended for those who are more immediately interested in the manufacture of painter's colours and varnishes; and the author, as he says in his preface, has in the treatment of his subject endeavoured to combine theory and practice by giving a short account of the theory of the processes which he describes. Considering the range of the work and the great number of subjects dealt with, the information thus afforded is necessarily here and there somewhat scanty, and hardly sufficient to work upon. At the same time it appears to us that the bulk of the book might with advantage have been reduced by leaving out most of the elementary chemistry, with which nowadays we must suppose anyone would be thoroughly familiar, who intended to embark on colour-making. A colour-maker ought, above all things, to be a thorough chemist if he wishes to succeed; and those who merely rely on the working of their stock of receipts, secret processes, and rule-of-thumb directions, will have but little chance of success in the future. The author, in his desire to make his work as complete as possible, has also given undue space to the description of numerous colours and processes which are now obsolete, and to others which attracted only a transient interest when issued from the Patent Office.

The uninitiated reader will therefore find it somewhat difficult to separate the chaff from the grain, as it is not always clear whether, in the description of the processes and the properties of the colours obtained by them, the author gives his own experience, or merely a transcript of the patent specification or the trade circular. This criticism applies more especially to the chapter on white pigments. Here it is also to be regretted that the author has not give more prominence to the description of the qualities of the typical whiteleads obtained by the different processes, and has not brought them into comparison with other similar lead compounds, and with the various zinc pigments which have been introduced as substitutes for the poisonous whitelead. The results of such comparative trials carried out systematically, and according to the methods mentioned in the book, would have been of great value, as they would also assign the proper value to those compounds which are so persistently recommended as being equal, or even superior, to genuine old-fashioned whitelead.

With regard to zincwhite, it appears to us that the real reason of its not being more generally employed is not its supposed want of covering power, for well-prepared Belgian zincwhite ground in oil is hardly inferior in this respect to whitelead, but that it does not work so smoothly and freely under the brush, however finely it may be ground. On this account the workman will always prefer to use whitelead, and he will only use zincwhite when he is compelled to do so. We notice that the author, in describing the preparation of zincwhite, says "the vapour of zinc burns, and vapour of zincoxide is emitted in large volumes"; and further on he speaks of the use of "zinc ores, such as calamine and zincblende, and any product of zinc which, being heated in a retort, can be reduced to the metal." Both these statements are absurdly incorrect. Here also we notice that the author, in referring to certain furnace operations, makes frequent use of the term "calcining," but quite indiscriminately, whether the operation is carried out on an open hearth, or in a muffle with the exclusion of air, or in a covered crucible. It is desirable that writers on technical matters should preserve the strict meaning of such terms as this, which have a distinct and specific signification.

The chapter on Barytes the author introduces with the following words:—"Barytes is one of the most important white pigments at the disposal of the painter, probably in this respect ranking next to whitelead." We have no hesitation in saying that this will surprise those who have any acquaintance with the behaviour of barytes when ground in oil or in varnish. It is true that this substance, as shown in the numerous analyses given in this book, enters largely into the composition of a good many commercial colours; but we venture to say

that, with a few exceptions, where the precipitated barium sulphate is used as a basis or bearer of certain colouring matters, the admixture of ground native barytes has no other object than that of adulteration. As an oil paint it is absolutely worthless. But while we have not a good word to say for ground native barytes, especially when it enters into the composition of oil colours, it is quite otherwise with the precipitated or artificial barium sulphate, which, in our opinion, receives here too scanty a treatment, judging by the little the author says about its preparation. It seems almost as if he had quite overlooked its vast importance. He omits to mention that this pigment is principally made from native barium carbonate, or Witherite, by what apparently is a very simple process; a process, however, which only in the hands of very few makers furnishes it in that perfect condition in which it finds so large an application in preparing the surface of paper for chromo-lithography. It is not, indeed, too much to say that this art largely owes its modern development to the introduction and use of this form of barium sulphate. Passing on to gypsum, we think, from the description given, the reader will receive quite an exaggerated idea of the qualities and importance of this substance in its application as a pigment. The author says "its body is not as good as that of white-lead, but it is at least equal to barytes in this respect, and is superior to zincwhite." He also states that it mixes well with either water or oil, and it can be mixed with all other pigments without affecting them. Here we might ask, are these the results of the author's own trials? In speaking of its being used very largely by paperstainers and makers of paper-hangings, he says it is preferred to barytes on account of its having more body when used for that class of work; "bulk," perhaps, would be a more correct term in this case. We almost suspect that the author, whilst speaking of the use of gypsum as a pigment, really means "satin-white," a substance of some importance to the paperstainer and maker of paper-hangings, which is obtained by precipitating aluminium sulphate with caustic lime, and which is a mixture of calcium sulphate and alumina hydrate. This substance, however, we do not find mentioned in the book before us.

It deserves to be noticed that, according to the author, gypsum as well as barytes and China clay are used in the finishing of cotton goods. We were under the impression that this nefarious and stupid practice had been given up before now. Strontium sulphate, under the name of strontian white, finds also a place amongst the white pigments, and considering the price of strontium minerals, we are not a little surprised to see it stated that strontium sulphate is often sent out as a substitute for barytes. Concluding with the white pigments, we find a very full and interesting account of China clay, and the mode of obtaining it from the natural deposits.

Regarding the article on Vermilion, it appears to us that the author is under an entire misapprehension as to the merits of the dry and wet methods of its preparation. It is incorrect to say that the Chinese product is finer and more brilliant in tone than that made in Europe. The vermilion made at present in Europe by the sublimation process is quite equal to, if not better than, the Chinese; but, as regards colour and fineness, the wet-process vermilion is far superior to either, and is superseding the former.

We should have thought it hardly worth while to mention that the chromates of mercury and silver had been proposed as red pigments, and still less the chromate of copper, which, according to the author, is a dark red (?) coloured body. Under chrome-green we notice that the author takes "Guignet's green" to be chromic oxide, whilst it really is a hydrate of chromium oxide, and the chemical formula he gives for its formation is, of course, quite wrong.

In the article on Ultramarine we notice misspellings of the names of authors, and R. Hoffmann, for instance, is throughout referred to as "Hofmann."

The foregoing remarks have already taken up so much space, that we cannot find room for our comments on the remaining chapters dealing with the colours, and we must conclude here, but not without calling attention to the latter part of the book, which treats of paint vehicles, oils, resins and varnishes, and gives a very full and interesting account of these materials. The description of the manufacture of oil varnishes is particularly valuable, as it gives an exact account of processes which are usually guarded with much secrecy.

BRITISH FUNGUS FLORA.

British Fungus Flora; a Classified Text-book of Mycology. By George Massee. Vols II. and III. (London: Geo. Bell and Sons, 1893.)

IT was originally estimated that this work would be completed in three volumes, but, as we pointed out in our first notice, this was practically impossible. A notice now accompanies the third volume, to the effect that a supplementary volume will speedily be published in conclusion of the work. The second volume continues the Hymenomycetes, which occupy 268 pages also of the third volume, so that two and a half volumes are occupied by the Hymenomycetes, leaving 220 pages for the Hyphomycetes, which bring the third volume to a close. It is only necessary to enumerate the remaining orders to appreciate the difficulty of completing even with a fourth volume. There are all the Ascomycetes, which occupied in Cooke's "Handbook" an equal number of pages to the Hymenomycetes; and supposing the increase to have been in the same ratio, it may be conjectured that this order (including the Discomycetes) cannot be compressed into less than two volumes. For all that remains afterwards, there will not be so much necessity. There would be the *Sphæropsideæ* and *Melanconieæ*, which are of minor interest, although numbering perhaps 700 species. The *Phycomyceteæ*, which have recently been the subject of a volume, by the same author, as "British Fungi, Phycomycetes, and Ustilagineæ." Hence they may be dispensed with. The *Uredineæ*, which, with the *Ustilagineæ*, formed a volume by C. B. Plowright in 1889, have had so few accessions that a revision is not imperative. The *Myxomycetes*, which occupied a monograph by G. Massee in 1892, is all-sufficient. As to the *Saccharomycetes* and the *Schizomycetes*, the little volume by W. B. Grove, dated 1884, would furnish an introduction, and would be fairly

complete to that date, in genera and species. With these modifications, we see no reason why, with two supplementary volumes to contain all the *Ascomycetes*, the five volumes might not be accepted as a fair approximation to a "British Fungus Flora."

As far as the *Basidiomycetes* are concerned, and these will occupy half the bulk of the volumes, even if extended to five, it will be conceded that they are of the greatest interest to the largest number of persons, and, moreover, that they are treated with all the fulness that such an important section demands. We cannot help regretting, however, whenever we are called upon to use the book, that the sequence of families and genera were inverted. The ample descriptions, under each species, will nevertheless atone for much, and justify the appropriation of half the volumes to their service.

The 220 pages which are devoted to the *Hyphomycetes* (or moulds), will be especially welcome to students of microscopical fungi, not only because they are arranged according to the most recent system—that proposed by Saccardo—but also on account of the very useful figures illustrative of the several genera. In our opinion, these are the most successful of the illustrations yet included in the present work. It would have been a great achievement, had it been possible, so to have increased the number of these little outline figures, as to have included every one of the species included in the Flora. As to substantial accuracy, that must, to a large extent, be accepted on trust, since the practical use can be the only test of the merits of the text-book, and its demerits—if any.

We can now estimate the number of British species, and see how they are provided for in this Flora, or may hereafter be included, viz.:—

Gastromycetes	78	... Vol. I.	
Hymenomycetes	1950	... Vols, I. II. III.	
Hyphomycetes	635	... Vol. III.	
			2663
Pyrenomycetes	900		
Discomycetes	610		
Hysteriacei	30		
Tuberaceæ	30		
			1570
Sphæropsideæ, &c.	700		
Uredineæ and Ustilagineæ	257		
Phycomycetes	100		
Myxomycetes	100		
Saccharomycetes and Schizomycetes	133		
			1290
Total			5523

The above estimates of the Ascomycetes, at 1570, are only approximate, and probably below, rather than above, the actual number. Hence therefore the total contents of the four or five volumes, as the case may be, would not be less than 4200 species, and the absolute total of all British recorded Fungi upwards of 5500 species, as compared with the 2809 of Cooke's "Handbook" in 1871, or a duplication in twenty-two years. This fact is a sufficient justification for the publication of the present work.

We need not repeat our general commendation, as expressed in our first notice, except perhaps to intimate that in all respects the second and third volumes are up to the level of the first, and justify the confidence reposed by the publishers, and ourselves, in the author to whom such an important work has been entrusted. It would be folly to pretend that it is absolutely perfect, but the errors of judgment, or execution, are not such as to detract from the general utility of the most pretentious and important work which Mr. Massee has yet attempted.

M. C. COOKE.

OUR BOOK SHELF.

Some Salient Points in the Science of the Earth. By Sir J. William Dawson, F.R.S. Pp. 499, with 46 illustrations. 8vo. (London: Hodder and Stoughton.)

THIS volume will have a melancholy interest, especially for the older geologists; for the author says that it "is intended as a closing deliverance on some of the more important questions of geology, on the part of a veteran worker, conversant in his younger days with those giants of the last generation, who, in the heroic age of geological science, piled up the mountains on which it is now the privilege of their successors to stand." We must bear in mind this implied limitation, that the heroic age of geology is now past, and must treat the volume before us as containing an account of researches and speculations made during the lifetime of a bygone generation. It is, in fact, a sort of scientific autobiography, touched up here and there to agree with recent research, but not claiming authority as an epitome of the present state of our knowledge.

Few chapters in the history of geology are so fascinating as Lyell's account of the discovery, by Principal Dawson and himself, of the wonderful series of remains from the coal field of Nova Scotia. It reads like the story of the exploration of a new country. We seem to walk among the strange vegetation of the coal; we see the larger reptiles crawling over and leaving trails in the soft mud; and on the dry land we help to pull to pieces one of the hollow trees, and find within it a number of land animals, all new to science. We can understand how these discoveries came as a complete surprise to the scientific world in days when few or no reptiles were known of earlier date than the Permian, and no land mollusca earlier than the Eocene. The reader will naturally turn first to the chapters relating to the researches in the coal measures, and it is probably on the observations there recorded that the author's fame will principally rest. Sir William Dawson's exploration of the coal measures of Nova Scotia led him to devote particular attention to the natural history of that period. He studied carefully the physical conditions under which the strata were laid down, devoting special attention to the formation of underclays and to the origin of coal seams; he still stands up for the dry-soil origin of coal, and for its growth on the spot. From the origin of coal it was a natural transition to the coal-measure plants, and these the author has worked at most industriously, though the present volume only contains a summary of his researches. There is also a chapter on the air-breathers of the coal, in which the author gives an account of his explorations at South Joggins.

We do not much care for the chapters on the genesis and migrations of plants, on the distribution of animals and plants as related to geographical and geological changes, and on Alpine and Arctic plants in connection with geological history. In all such subjects the author's strong bias against evolution in any form leads him to use strange arguments. We do not wish, however, to conclude this notice with a criticism of minor points, and though unprepared altogether to praise Sir William Dawson's volume, we thoroughly recognise how much he has done for the science of geology, and we gladly welcome in this handy form a short record of his life's work.

Das Karstphänomen. Versuch einer morphologischen monographie von Dr. Jovan Cvijić. (Wien: Ed. Hölzel.)

NOTWITHSTANDING its rather pompous second title, this is an interesting and valuable book, which, however, is not a separate work, but the third part of the fifth volume of a geographical series (*Abhandlungen*) edited by Prof. A. Penck. Its subject may be briefly stated as follows:—In many limestone districts the surface of the rock is guttered by channels—sometimes small, sometimes large—varying from comparatively smooth to rough. Here each ends in a small pipe, which descends vertically into the rock; there they converge towards one of larger size. With this system of superficial drainage are associated hollows of various forms, "blind valleys," and the like, and caves are likely to be common. A region which exhibits some or all of these phenomena is called, from the peculiar sculpture of the surface, a *karst* region. Such may be found in various parts of the world. It is represented in England by the furrowed limestones and "swallow-holes" of Derbyshire and Yorkshire; it occurs in many parts of the Alps, the phenomena becoming more frequent eastward, till their headquarters are reached in the Julian Alps and the great "Karst plateau," north of the Gulf of Fiume. As they occur in many lands, so they bear many names. A full, exhaustive, and elaborate account of these interesting phenomena will be found in this memoir, perhaps with an affected attempt at precision in distinction and classification (for after all, though curious, they are simple in origin), together with abundant references to the literature of the subject. Its usefulness, however, would be greatly increased by an index or by a very full table of contents; and though it is paged continuously with the volume, the latter, at least, ought to have been given.

T. G. B.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Origin of Lake Basins.

THE most thorough-going glacialist could find no ground for complaint that Dr. Wallace has not gone far enough in his most interesting advocacy of the glacial origin of lakes. I do not propose to enter into any general discussion of this question; that glaciers can excavate rock basins is indisputable, but there is a limit to their power, and this limit I believe to be reached far short of even the larger of our English lakes. The controversy is of long standing, and there is little new to be said on either side; nor would I have desired to re-enter it, but that Dr. Wallace's article seems to me to contain one serious fallacy and one vital misstatement which have not as yet been noticed, though they should not be left uncorrected.

The fallacy is not a new one; it may be found in the writings of more than one of the advocates of the glacial theory, and is contained in the argument that because lakes are found in regions that have been extensively glaciated, and are not found in regions precisely similar in every respect, except that there has been no great extension of glaciers, therefore the rock basins in which the lakes lie were excavated by glaciers. I trust I have not misrepresented the argument in this succinct statement of it; but such condensation is useful if we would detect a fallacy, and in this condensed form the fallacy of the undistributed middle term becomes conspicuous. The term "lake" is by no means coextensive with the term "rock basin," and it is not the water filling the lake which requires explanation so much as the basin that it fills. A rock basin filled with alluvium is a rock basin still, and requires explanation as much as if it contained water, and was consequently a lake.

The misrepresentation is to be found in Dr. Wallace's limitation of what he rightly regards as the only tenable alternative theory, that the rock basins owe their origin to deformation of the surface immediately before the advance of the ice. This limitation of time is so extraordinary that it would have passed for an accident or oversight, but that it is repeated at greater length on the very next page; did it form any essential part of the theory, this would deserve all the strictures passed upon it, but such is by no means the case. Without entering into the question of whether the geologists quoted by Dr. Wallace accept this limitation of time, I may point out that it is altogether more reasonable to regard the deformation as having taken place *after* the advance of the glaciers. We know that during the glacial period there were great changes of level, and it is reasonable to suppose that these were not absolutely uniform; moreover, had the rock basins been formed before the ice was there to fill them, they would mostly have been filled at once by river deposits, as has been the case in nonglaciated regions, and once filled up they would remain so on this theory, for if a glacier cannot erode a rock basin it cannot clean out one that has been filled up with stream deposits. This alteration of time makes the theory more natural and acceptable; when a rock basin is formed in the course of a stream by elevation or subsidence no lake arises in the great majority of cases, as either the barrier is destroyed by corrasion, or the hollow is filled up by deposition, as fast as it is formed; but when the basin arises underneath a glacier it becomes occupied by ice, and on the retreat and disappearance of the glacier a hollow is left that is at first filled by water, forming a lake, and only subsequently by degrees filled up by stream deposits. In this way the connection between the present distribution of lakes and the areas of pleistocene glaciation is easily explicable, and it is consequently not admissible as an argument to prove that the lake basins were excavated by glaciers until it is shown that in the nonglaciated regions, where there are now no lakes, there are also no rock basins.

With most of the regions quoted by Dr. Wallace I have no personal acquaintance, but in India such do certainly occur, and have as certainly not originated by glacial erosion; in some cases the existence of the rock basin has been proved by borings, but besides these there are many more instances where there can be no reasonable doubt of the existence of a rock basin, though the final test has not been made.

R. D. OLDHAM.

IN his last communication Sir Henry Howorth makes two statements which are so erroneous and so misleading that I cannot allow them to pass without correction. The first is, that Mr. Deeley "repudiates Dr. Wallace's notion that regelation can in some way act as a compensating element when crushing supervenes in ice." Here is a double misstatement. Mr. Deeley "repudiated" no notion of mine, or he would, I am sure, have said so plainly, and he said nothing whatever about "crushing." Neither did I say a word about regelation acting as a "compensating element," for I do not believe in the crushing of glaciers by their own pressure. I asked Sir Henry what would happen to the ice after it was crushed, the pressure continuing; and I get no reply but the above double misstatement.

Then, further on, Sir Henry says: "Mr. Wallace confesses he does not like to face these mechanical issues." This is simply untrue. I "confessed" nothing of the kind, and I challenge Sir Henry Howorth to quote any words of mine which will bear such a meaning. I maintain that his "mechanical issues" are pure theories, and are beside the question of the actual facts of glacier notion. Lastly, he attempts to evade the real issue between us, which is, that he himself accepted Charpentier's conclusions as to the extent of the Rhone glacier, but refuses to allow me to use these same conclusions as a datum in the discussion.

I have now shown ample reason why further discussion of this matter with Sir Henry Howorth must be unprofitable.

ALFRED R. WALLACE.

The Second Law of Thermodynamics.

I AM unable to see any reason for regarding Clausius' supposed deduction of the Second Law as in any way limited by the condition stated by Mr. Burbury, viz. "that the system be conservative, that is, that the external as well as the internal forces acting on it are to be derived from a potential." No such limitation was contemplated by me when I was preparing the Report for the British Association.

It is true that this assumption is made in § 17 of the Report, in order to establish the closest possible connection between the

deduction and Hamilton's Principle of Least Action. But the limitation is required by the postulate, viz. the Principle of Least Action, not by the deduction. Clausius expressly states (*Phil. Mag.* vol. xliv. (1872) p. 365) that his deduction holds good for systems to which the Principle of Least Action is not directly applicable, and in consequence he claims that his equations involve a new principle which is of more general application than Hamilton's Principle (*vide* Report, § 16). I think there can be little doubt that Clausius had in his mind the very objection which Mr. Burbury now raises, and that it was in order to meet it that he claimed this generalisation.

The assumption as to the conservative nature of the forces is not required except in § 17 of the Report, and at the end of that paragraph two methods of avoiding it are suggested. One is to assume that the force-function can be varied with the time, the other is a method adopted by Von Helmholtz.

If we allow the force-function to be varied with the time, then in Mr. Burbury's case (of a column of gas held down by a piston of constant mass) the potential of *gravity* can be altered and therefore the *weight* of the piston is disposable. This disposes of Mr. Burbury's objection, and it only remains to consider the investigations given at the beginning of § 17 of the Report.

Clearly the Second Law of Thermodynamics cannot be deduced from studying the behaviour of gas under constant pressure. To establish it we must make the working substance undergo a reversible cycle in which heat is absorbed and external work performed. To do this we attach the piston to a crank as in an ordinary steam engine, and make it turn a wheel which raises a weight by means of a windlass. Here we have a strictly conservative system, and one to which the arguments of § 17 are therefore strictly applicable. And for every single turn of the wheel we have

$$\int \frac{dQ}{T} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

a relation identical in form with that which expresses the Second Law.

It seems to me that the real objections to Clausius' deductions are far more intricate and far less easily disposed of. The difficulty of assigning a physical meaning to the quasi-period i is one of them, and there are other difficulties connected with the interpretation of $\bar{T}$ as absolute temperature when intermolecular forces are taken into account. All these difficulties are alluded to in my Report, and they are not peculiar to the hypothesis of "quasi-periodic" motions; similar difficulties exist in some form or other in most so-called "proofs of the Second Law."

It may be interesting to mention that a proof of the Second Law based on the virial equation

$$pv = \tfrac{2}{3}(T + \Sigma\Sigma(\tfrac{1}{2}Rr))$$

was given by R. C. Nichols in the *Philosophical Magazine* for 1876 (v. Series I. p. 369). I hope later on to deal more fully with that portion of Mr. Burbury's letter which relates to the "virial proof."

G. H. BRYAN.

December 21.

Flame.

I HAD hoped that after disavowing the unpleasant interpretation which had been put upon his first letter, Dr. Armstrong would have done me the honour, and himself the justice, of indicating precisely where he disagreed with my scientific arguments. Instead of doing so, he has imputed to me a sensitiveness to criticism so excessive that he feels it best to retire from the controversy with a mere statement that our standpoints are different. I must leave it to the readers of NATURE to judge whether Dr. Armstrong has any longer the right to claim a *standpoint*.

Mr. Newth will find a fuller discussion of my views about flame in the *Journal of the Chemical Society* for 1892, pp. 204–226. If after reading that he still has difficulty in understanding the fundamental points of my work, I shall be glad to help him if he will communicate with me privately. With a little care, Mr. Newth will find it quite easy to separate the two cones of a carbon monoxide-air flame in the ordinary apparatus without the use of a gauze cap. The air must be turned on very gradually. In the case of the hydrogen-air flame it is best to dilute the gases with nitrogen, as recommended in my first paper.

I am sorry that anyone should think I have slighted Dr. Frankland's work. I can, however, understand, and even admire, Mr. Newth's excessive zeal in the matter.

ARTHUR SMITHELLS.

"The Zoological Record."

WE are delighted to find such a consensus of opinion as to the desirability of retaining palæozoology in the *Zoological Record*. The recorders whom we have consulted, the editor, and now the secretary of the Zoological Society, all have expressed themselves in its favour. The question therefore is purely one of finance. Under these circumstances the publication of the correspondence with the Geological Society is of great interest, and the only addition that we could suggest would be the publication in your columns of that poverty-stricken society's balance-sheet.

We should like, however, to point out that the *Zoological Record* appeals to the Geological Society, not merely on the ground of its palæontological contents. Palæontologists go to the *Record* to learn what the neontologists are doing, quite as much as to read the titles of their own papers. Under any circumstances, then, the *Zoological Record* has some claim on the Geological Society, and we must all regret that financial distress prevents the Society from acknowledging that claim.

But the Record Committee of the Zoological Society need not despair; for the *Record* has no less claims on many other of our learned societies, and, by the converse argument, the inclusion of palæozoology merely strengthens those claims. Every biologist should be grateful to those who bring to his notice literature that he would otherwise never hear of. Apart from this, one-third of the volume is devoted to entomology. Why should the Entomological Society not be invited to contribute? Then there are the Royal and the Linnean Societies, and the British Association; the Microscopical, we would mention, did it not already do excellent work of a similar kind. At any rate, surely five of these bodies could be prevailed on to subscribe £20, or even £40 a-piece. The Zoological Society appeals through its *Record* to hundreds of workers who do not belong to it. It has long done an admirable work, of which it will never lose the credit. Everyone should sympathise with it in its present attempt to perfect this work, and should not permit it to suffer so large a pecuniary loss in that attempt.

December 17.

R. I. POCOCK.
F. A. BATHER.

On the Bugonia-Superstition of the Ancients.

LAST August, I published in the *Bulletin Soc. Entomol. Italiana*, 1893, p. 186–217, an article entitled "On the Bugonia of the Ancients, and its relation to *Eristalis tenax*, a two-winged insect." I desire to collect some more materials on that subject, in view of a second edition, and I would be very grateful to readers of NATURE who may be able to give me assistance in that matter.

The information I require may be expressed in two questions:

(1) Whether travellers in out-of-the-way places in Europe or Asia have not come across vestiges of the superstition about oxen-born bees, still lingering among primitive people?

(2) Whether readers of Oriental literature have not come across passages evidently referring to this superstition, like the passage I reproduce here as an example. I found it in the "Golden Meadows" of the Arab traveller Massoudi (died in Cairo, 955), translated by Barbier de Meynard and Pavet de Courteille, Paris, 1861, vol. iii. p. 233. It relates a conversation which took place in Arabia, and of which this is a fragment. "'Had the bees, which produced this honey, deposited it in the body of a large animal,' asked Yiad? The surveyor answered: 'Hearing that there was a hive near the sea-coast, I sent people to gather the honey. They told me that they found at that place a heap of bones, more or less rotten, in the cavity of which bees had deposited the honey that they brought with them.'"

I have sent separate copies of my paper to the Geographical, Linnean, and Entomological Societies in London, to the Natural History Museum, South Kensington, to the Athenæum Club, and to many friends in England. I should be happy to send a copy to anybody interested in the subject.

C. R. OSTEN SACKEN.

The Earliest Mention of the Kangaroo in Literature.

I TAKE advantage of the present opportunity to put another question to zoologists. In the same book of Massoudi, whom I quoted in the previous notice, I found the following passage (vol. i. p. 387):—"El Djahiz, in his 'Book on Animals,' relates that the female rhinoceros is pregnant for seven years,

during which the cub protrudes the head from the belly of the mother, in order to browse, and withdraws it afterwards. Desirous of being better informed, I asked the people of Siraf and Oman, who visited this country, as well as merchants whom I had met in India. They all told me that the rhinoceros breeds just like the cow and the buffalo; and I do not know where El Djahiz has found this story, whether among his reading, or from his inquiries." This is evidently an obscure tradition about the Australian kangaroo, which had reached some part of Asia, and was connected with the rhinoceros by people who knew nothing about either of the two animals. Has the attention of zoologists been called to this story before?

Heidelberg, Germany, December 5. C. R. OSTEN SACKEN.

On an Undescribed Rudimentary Organ in Human Attire.

LECTURERS who are tired of the cockade hat-ribbon and tail buttons, may be glad to know of the following rudiment. The old-fashioned double eye-glass was a folder, with a knob at the outer side of the distal glass; and this on folding locked against a pin on the outer side of the proximal glass. The double eye-glass of the present day does not fold; but, none the less, is the knob outside the distal glass retained for it, though there is no pin to lock with on the proximal glass. How long will it take before this useless rudiment disappears? What will be the cause of its disappearance? As panmixia is out of the question, we may prophesy that it will be economy of material.

Cork, December 12. MARCUS HARTOG.

EARLY ASTERISMS.[1]

III.

The Constellations referred to in the Myth of Marduk and Tiāmat.

WE are indebted to the myth, then, for the knowledge that when it was invented the constellations Bull, Scorpion, Goat, and Fishes had been established.

This argument is strengthened by the following considerations suggested by Jansen:—

"We look in vain among the retinue of Tiāmat for an animal corresponding to the constellations of the zodiac to the east of the vernal equinox. This cannot be accidental. If therefore we contended that the cosmogonic legends of the Babylonians stood in close relationship to the phenomena of sunrise on the one hand and the entrance of the sun into the vernal equinox on the other; that, in fact, the creation legends in general reflect these events, there could not be a more convincing proof of our view than the fact just mentioned. The three monsters of Tiāmat, which *Marduk* overcomes, are located in the 'water-region' of the Heavens, which the Spring Sun *Marduk* 'overcomes' before entering the (ancient) Bull. If, as cannot be doubted, the signs of the zodiac are to be regarded as symbols, and especially if a monster like the goat-fish, whose form it is difficult to recognise in the corresponding constellation, can only be regarded as a symbol, then we may assume without hesitation that at the time when the Scorpion, the Goat-Fish, and the Fish were located as signs of the zodiac in the water-region of the sky, they already played their parts as the animals of Tiāmat in the creation legends. Of course they were not taken out of a complete story and placed in the sky, but conceptions of a more general kind gave the first occasion. It does not follow that all the ancient myths now known to us must have been available, but certainly the root-stock of them, perhaps in the form of unsystematic and unconnected single stories and concepts."

There is still further evidence for the constellation of the Scorpion.

Jensen remarks:—

"A Scorpion-Man plays also another part in the cosmology of the Babylonians. The Scorpion-Man and his wife guard the gate leading to the Māšu mountain(s), and watch the sun at rising and setting. Their upper part reaches to the sky, and their *irtu* (breast?) to the lower regions (Epic of Gistubar 60,9). After Gistubar has traversed the Māsu Mountain, he reaches the sea. This sea lies in the east or south-east. However obscure these conceptions may be, and however they may render a general idea impossible, one thing is clear, that the Scorpion-Men are to be imagined at the boundary between land and sea, upper and lower world, and in such a way that the upper or human portion belongs to the upper region, and the lower, the Scorpion body, to the lower. Hence the Scorpion-Man represents the boundary between light and darkness, between the firm land and the water region of the world. *Marduk*, the god of light and vanquisher of Tiāmat, *i.e.* the ocean, has for a symbol the Bull = Taurus, into which he entered in spring. This leads almost necessarily to the supposition that both the Bull and the Scorpion were located in the Heavens at a time when the sun had its vernal equinox in Taurus and its antumnal equinox in Scorpio, and that in their principal parts or most conspicuous star groups; hence probably in the vicinity of Aldebaran and Antares, or at an epoch when the principal parts of Taurus and Scorpio appeared before the sun at the equinoxes."

If my suggestion be admitted that the Babylonians dealt not with the daily fight but with the yearly fight between light and darkness—that is, the antithesis between day and night was expanded into the antithesis between the summer and winter halves of the year; then it is clear that at the vernal equinox Scorpio setting in the west would be watching the sunrise; at the autumnal equinox rising in the east, it would be watching the sunset; one part would be visible in the sky, one below the horizon in the celestial waters. If this be so all obscurity disappears, and we have merely a very beautiful statement of a fact, from which we learn that the time to which the fact applied was about 3000 B.C., if the sun were then near the Pleiades.

Jensen in the above-quoted passages by implication, and in a subsequent one directly, suggests that not all the zodiacal constellations were established at the same time. The Babylonians apparently began with the easier problem of having six constellations instead of twelve. For instance, we have already found that to complete the present number, between

Scorpio. Capriconus. Pisces.

we must interpolate

Sagittarius. Aquarius.

Aries and Libra seem also to be late additions according to Jensen, who writes:—

"We have already above (p. 90), attempted to explain the striking phenomenon that the Bull and Pegasus, both with half bodies only, ἡμίτομοι, enclose the Ram between them, by the assumption that the latter was interposed later on, when the sun at the time of the vernal equinox stood in the hind parts of the Bull, so that this point was no longer sufficiently marked in the sky. Another matter susceptible of a like explanation may be noted in the region of the sky opposite to the Ram and the Bull. Although we cannot doubt the existence of an eastern balance, still, as already remarked (p. 68), the Greeks have often called it χηλαὶ 'claws' (of the Scorpion), and according to what has been said above (p. 312), the sign for a constellation in the neighbourhood of our Libra reads in the Arsacid inscription 'claw(s)' of the Scorpion. These facts are very simply explained on the supposition that the Scorpion originally extended into the region of the Balance, and that originally α and β Libræ represented the 'horns'

[1] Continued rom vol. xlviii. p. 520.

of the Scorpion, but later on, when the autumnal equinox coincided with them, the term Balance was applied to them. Although this was used as an additional name, it was only natural that the old term should still be used as an equivalent. But it also indicates the great age of a portion of the zodiac."

Let us suppose that what happened in the case of Aries and Libra happened with six constellations out of the twelve, *in other words, that the original zodiac consisted only of six constellations.* We should have—

Taurus		Crab (or Tortoise)		Virgin (or ear of corn)		Scorpion		Capricornus		Pisces	
	Gemini		*Lion*		*Libra*		*Sagittarius*		*Aquarius*		*Aries*

The upper list not only classifies in an unbroken manner the Fish-Man, the Goat-Fish, the Scorpion-Man, and Marduk of the Babylonians, but we pick up all or nearly all of the ecliptic stars or constellations met with in early Egyptian mythology, Apis, the Tortoise,[1] Min, Selk, Chnemu as represented by appropriate symbols.

Further, the remarkable suppression or small representation of the Lion in both the more ancient Babylonian and Egyptian mythology is explained. I have shown before how the Babylonians with an equinoctial year would take slight account of the solstice, while it also follows that the Egyptians, who were wise enough not to use zodiacal stars for their warnings of sunrise for the reason that stars in the brighter light of dawn near the sun are more difficult to see, might easily neglect the constellation of the Lion as first Phact and then Sirius, both southern stars, marked for them the advent of the summer solstice; on different grounds, then the Lion might well have been at first omitted in both countries.

Since there is a doubt as to the existence of the Lion among the first Babylonian constellations,[2] the argument in the following paragraph would appear to refer to observations made at a later time when totemism was less prevalent:—

"The Lion in the heavens must represent the heat of the summer. He does this most effectually when the summer solstice coincides with the constellation, that is, when its principal stars appear before the sun at the summer solstice. This happened at the time when the vernal equinox lay in Taurus, and when the principal star-group of the Bull appeared before the sun at the time of the vernal equinox. The water-jug (Amphora)—Aquarius must represent symbolically the watery season of winter. It does this most effectually when the winter solstice coincides with it, or its principal star-group appears before the sun at the winter solstice. This happened about the time when the vernal equinox lay in Taurus, and its principal star-group rose before the sun at the time of the vernal equinox."

The above suggested basis of the Babylonian mythology, regarding the demons of Tiāmat, established when the sun was in Taurus at the spring equinox, enables us to understand clearly the much later (though similar) imagery employed when the sun at the equinox had passed from Taurus to Aries—when the Zend Avesta was written, and after the twelve zodiacal constellations had been established. We find them divided equally into the kingdoms of Ormuzd and Ahriman. Here I quote Dupuis:[1]

"L'agneau est aux portes de l'empire du bien et de la lumière, et la balance à celles du mal et des ténèbres; l'un est le premier des signes supérieurs, et l'autre des signes inférieurs.

"Les six signes supérieurs comprennent les six mille de Dieu, et les six signes inférieurs les six mille du diable. Le bonheur de l'homme dure sous les premiers signes, et son malheur commence au septième, et dure sous les six signes affectés à Ahriman, ou au chef des ténèbres.

"Sous les six signes du règne du bien et de la lumière qui sont agneau, taureau, gémaux, cancer, lion et vierge ou épi nous avons marqué les états variés de l'air et de la terre, qui sont le résultat de l'action du bon principe. Ainsi on lit sous l'agneau ou sous le premier mille ces mots, printemps, zephyr, verdure; sous le taureau, sève et fleur; sous les gémeaux, chaleurs et longs jours; sous le cancer, été, beaux temps; sous le lion, épis et moissons; et sous la vierge, vendages.

"On passant à la balance, on trouve les fruits; là commence le règne du mal aussitôt que l'homme vient à cueiller les pommes. La nature quitte sa parure; aussi nous avons écrit ces mots. Dépouillement de la nature, sous le scorpion on lit froid; sous le sagittaire, neiges; sous le capricorne, glace et brouillard, siége des ténèbres et de long nuits; sous le verseau, pluies et frimas; sous les poissons, vents impétueux."

We now return for a moment to Ia.

Associated with Ia was an Ia-star, which Jensen concludes may be η Argûs. This we must consider.

Jensen concluded that the Ia-star is η Argûs, on the ground that many of the texts suggest a darkening of it now and again; he next proceeds to point out that a variability in the star is the only point worth considering in this connection, and by this argument he is driven to η, which is one of the most striking variables in the heavens, outshining Canopus at its maximum. Speaking generally, everybody would agree that observation by clouds, &c., would not be recorded, but if the star were observed just rising above the southern horizon only, then its absence, due to such causes, would, I should fancy, be chronicled, and it must not be forgotten that this is precisely the place where it would be observed, for in the first place it was to the south of the heavens, what Bil was to the north, and the temple sacred to it at Babylon was oriented to the south.

But η Argûs never rose or set anywhere near the south. I have ascertained that its declination was approximately $33\frac{1}{2}°$ S. in 6000 B.C., and increased to 40° S. by about 2000 B.C. Hence between these dates at Eridu its amplitude varied between 38° and 47° S. of E. or W. Now here we are far away from the S. point, though very near the S.E. or S.W. point, to which it is stated the Babylonian structures had their sides oriented.

The question arises whether there was a star which answers the other conditions. There was a series of such stars. First, beginning with the most recent; we have *Canopus.* 6000 B.C. its declination was $62\frac{1}{2}°$, it would then have been below the horizon of Eridu, first making its appearance with a declination of 59° nearly at the south point in 4700 B.C. Phact would follow in 5400 B.C. Achenar would make a similar appearance for the first time about 8000 B.C. It may be here mentioned generally that the precessional movement must, after certain intervals, cause this phenomenon to be repeated constantly with one star after another. May this explain the "other animals" who subsequently appeared like Ia (Oannes)? The whole myth is, I think, clearly one relating to men coming (from the south?) to Eridu in ships. The boat is turned into a "fish man," and the

[1] I think I am right about the Tortoise, for I find the following passage in Jensen, p. 65, where he notes the absence of the Crab:—"Ganz absehend davon, ob dasselbe für unsere Frage von Wichtigkeit werden wird oder nicht, muss ich daran erinnern, das unter den Emblemen, welche die sogenannten "Deeds of Sale" häufig begleiten, verschiedene Male wie der Scorpion so die Schildkröte abgebildet gefunden wird. . . ."

[2] Jensen, p. 314.

[1] "Origine des Cultes," vol. vii. p. 82.

star to which they pointed to show whence they came or made a god.[1]

It will have been gathered that the constellations of the Bull and the Scorpion were recognised as such at the same early date both in Babylonia and Egypt, and this of course implies intercommunication.

The ecliptic stars in use in Babylonia in later times are as follows [2]:—

1.	η Piscium.	15.	α Leonis.
2.	β Arietis.	16.	ρ Leonis.
3.	α Arietis.	17.	β Leonis.
4.	η Tauri.	18.	β Virginius.
5.	α Tauri.	19.	γ Virginius.
6.	β Tauri.	20.	α Virginius.
7.	ζ Tauri.	21.	α Liliæ.
8.	η Geminorum.	22.	β Liliæ.
9.	μ Geminorum.	23.	δ Scorpionis.
10.	γ Geminorum.	24.	α Scorpionis.
11.	α Geminorum.	25.	δ Ophiuchi.
12.	β Geminorum.	26.	α Capricornis.
13.	δ Cancri.	27.	γ Capricornis.
14.	ϵ Leonis.	28.	η Capricornis.

With regard to the complete ecliptic, the information seems meagre both from Babylonia and from Egypt in early times.

As to later times in Babylonia—say 1000 B.C.—the following list represents the results of Jensen's investigations :—

(1) Perhaps Aries (= "leading sheep").
(2) A "Bull (of the Heavens)" = Aldebaran or (and) = our Taurus.
(3) Gemini.
(4) ?
(5) Perhaps Leo.
(6) The constellation of the "corn in ears" = the ear of corn. [Spica.]
(7) Probably Libra, whose stars are, however, at least in general, called "the claw(s)" (*i.e.* of the Scorpion).
(8) The Scorpion.
(9) Perhaps Sagittarius.
(10) The "goat fish" = caper.
(11) ?
(12) The "Fish" with the "Fish band."

In Egypt we find no such sharp references as the above to either the poles or the great circles, but dating from the twentieth dynasty (1100 B.C.), and therefore almost contemporaneous, is a series of star tables which have puzzled Egyptologists from Champollion and Biot downwards.

Looking at them they seem to be observations of stars made during the twelve hours of the night on the 1st and 16th of every month. The chief stars seem to be twenty-four in number, and it looked at first as if we had really here a list of priceless value of twenty-four either ecliptic or equatorial stars.

Unfortunately, however, the list has resisted all efforts to completely understand it. Whether it is a list of risings or meridian passages even is still in dispute. Quite recently, indeed, one of the investigators, Herr Gustav Bilfinger,[3] has not hesitated to consider it not a list of observations at all, but a compilation for a special purpose.

"The star-table is intended to carry the principle of time into the rigid world of the grave, and represents over the sepulchral vault, 'the eternal horizon' as the ancient Egyptians so aptly styled the grave, an imitation of the sky, a compensation for the sky of the upper world with its time-measuring motion ; yet the idea here is bolder, the execution is more artificial and complicated, since the sculptor endeavoured to combine the daily and the annual motion of the celestial vault in *one* picture ; wanted to transfer into the grave the temporal frames in which all human life is enacted. This endeavour to represent by one configuration both motions and both chronological units explains all the peculiarities and imperfections of our star-table.

"The simplest means of representing both motions was found in the stars, which circle the earth in the course of a day and indicate the year by the successive appearance of new stars in the morning twilight. If the same stars were to serve both purposes in one representation, it was necessary to take twenty-four stars which rose at intervals of fifteen days, since only such followed each other at an average distance of 15°, and were therefore useful for showing the hours."

"If the calendar-maker really possessed a list of the twenty-four principal (zodiacal) stars, the course of the year was indicated thereby ; but since he also wanted to represent the daily motion, he might with some justice have composed each night out of eleven of these stars, since the stars' risings are only visible during the ten middle hours of the night. But ten hours would not have adequately represented the night, since this was thought of as a twelve hours' interval.

"There was a way out of it, viz. to call hora 0 'sunset,' hora 12 'sunrise,' which would have been a simple and correct solution if the division of the night into twelve parts for practical purposes had been aimed at. But this expedient he could not adopt, because he could or would only operate with stars, and the notions of sunrise and sunset found no place in his tables. Thus he was forced to *falsify* the customary division of the hours, by squeezing the twelve hours of the night into the time during which star risings are visible, viz. the dark night exclusive of twilight. On the other hand he could not, with his principal stars at intervals of 15°, divide his night, shortened as it was by two hours, into twelve parts, and thus he was obliged to make use of two or three auxiliary stars, as we have proved in detail above, and thus yet more to disfigure the hour-division, since thereby the lengths of the hours were made very variable. These are then two things which we must not regard as peculiarities of ancient Egyptian reckoning, but as a consequence of the leading idea of our table, which did not intend to facilitate the division of the night into twelve parts by star observations, but was calculated by the connection of thirteen stars with thirteen successive moments to create the idea of the circling host of stars and thence the course of the night."

I give an abstract of the list of the twenty-four principal stars and the constellations in which they occur :—

1.	Sahu = Orion.
2.	Gothis = Sirius.
3.	The two stars.
4.	The stars of the water.
5.	The lion.
6.	The many stars.
7.	Mena's herald.
8.	Mena.
9.	Mena's followers.
10.–14.	Hippopotamus.
15.–20.	Necht.
21.	Ari.
22.–23.	Goose.
24.	Sahu = Head of Orion.

It will be seen that this Egyptian star list is very indeterminate, but there are other lists, which are much more definite, represented by the Indian Nakshatras, the Arab Manâzil al-Kamar, and the Chinese Sieu.

[1] For the story as told by Bêrôssas, see Sayce, p. 131.
[2] "Astronomisches aus Babylon," pp. 117-133.
[3] "Die Sterntafeln in den ägyptischen Konigsgräbern von Bibân el Molûk".—von Gustav Bilfinger (p. 69).

Hindu Asterism.	Arab Manzil.	Chinese Sieu.
1. Açvinî (The two gods) β and γ Arietis	1. ash-Sharatân (The two signs) β and γ Arietis	1. Mao η Tauri
2. Bharanî (Carrying away) 35, 39, and 41 Arietis	2. al-Buṭain (The little belly) 35, 39, and 41 Arietis	2. Pi ϵ Tauri
3. Kṛttikâ (Has been explained as matting; doubtful) η Tauri, &c. (Pleiades)	3. ath-Thuraiyâ (Probably "the cluster") η Tauri, &c. (Pleiades)	3. Tse λ Orionis
4. Rohinî (Red) $\alpha, \theta, \gamma, \delta, \epsilon$ Tauri	4. ad-Dabarân ("The follower" of the Pleiades) $\alpha, \theta, \gamma, \delta, \epsilon$ Tauri	4. Tsan δ Orionis
5. Mṛgaçiras (Head of deer) λ, ϕ^1, ϕ^2 Orionis	5. al-Hak'ah (The circle of hairs) λ, ϕ^1, ϕ^2 Orionis	5. Tsing (A well) μ Geminorum
6. Ârdrâ (Damp) α Orionis	6. al-Han'ah (Apparently "the wishing Asterism") $\eta, \mu, \nu, \gamma, \xi$ Geminorum	6. Kuei θ Cancri
7. Punarvasu (Twice bright) β, α Geminorum	7. adh-Dhirâ' (The arm) β, α Geminorum	7. Lieu (The willow) δ Hydræ
8. Pushya (Auspicious) θ, δ, γ Cancri	8. an-Nathrah ("The point between lip and nostrils" of Leo) γ, δ Cancri, and Præsepe	8. Sing (A star) α Hydræ
9. Âçleshâ (Embracing, serpents) $\epsilon, \delta, \sigma, \eta, \rho$ Hydræ	9. aṭ-Ṭarf ("The eyes" of Leo) ξ Cancri, λ Leonis	9. Chang ν^1 Hydræ
10. Maghâ (The strong ?) $\alpha, \eta, \gamma, \zeta, \mu, \epsilon$ Leonis	10. aj-Jabhah (The forehead) $\alpha, \eta, \gamma, \zeta$ Leonis	10. Y α Crateris
11. Pûrva Phalgunî (Grey) δ, θ Leonis	11. az-Zubrah (The shoulder) δ, θ Leonis	11. Chin γ Corvi
12. Uttara Phalgunî β, 93 Leonis	12. aṣ-Ṣarfah ("The change" of weather) β Leonis	12. Kio (A horn) α Virginis
13. Hasta (Hard) $\delta, \gamma, \epsilon, \alpha, \beta$ Corvi	13. al-Auwâ ("The howler," sometimes conceived as a dog barking round Virgo) $\beta, \eta, \gamma, \delta, \epsilon$ Virginis	13. Kang (Overbearing, strong) κ Virginis
14. Citrâ (Beautiful) α Virginis	14. as-Simâk (The prop) α Virginis	14. Ti (A foundation) α^2 Libræ
15. Svâtî α Bootis	15. al-Ghafr (Of uncertain sense) ι, κ, λ Virginis	15. Fang (Room, dwelling) π Scorpionis
16. Viçâkhâ (Fork) $\iota, \gamma, \beta, \alpha$ Libræ	16. az-Zubânân ("The two claws" of the scorpion) α, β Libræ	16. Sin (The heart) σ Scorpionis
17. Anurâdhâ (Blissful) δ, β, π Scorpionis	17. al-Iklîl (The crown) β, δ, π Scorpionis	17. Uei (High) μ^2 Scorpionis
18. Jyeshṭhâ (The best) α, σ, τ Scorpionis	18. al-Ḳalb (The heart) α Scorpionis	18. Ki γ^2 Sagittarii
19. Mûla (Root) $\lambda, \upsilon, \kappa, \iota, \theta, \eta, \zeta, \mu, \epsilon$ Scorpionis	19. ash-Shaulah ("The sting" of the scorpion) λ, υ Scorpionis	19. Teu ϕ Sagittarii
20. Pûrva-Ashâdhâ (Unconquered) δ, ϵ Sagittarii	20. an-Na'âim (The ostriches) $\gamma^2, \delta, \epsilon, \eta, \phi, \sigma, \tau, \zeta$ Sagittarii	20. Nieu β Capricorni
21. Uttara-Ashâdhâ (Unconquered) σ, ζ Sagittarii	21. al-Baldah (The hairless space between the eyebrows) N of π Sagittarii	21. Nü ϵ Aquarii
22. Abhijit (Victorious) α, ϵ, ζ Lyræ	22. Sa'd adh-Dhâbiḥ (Sa'd (luck) the sacrificer) α, β Capricorni	22. Hiü β Aquarii
23. Çravana (Lame) α, β, γ Aquilæ	23. Sa'd Bula' ("Greedy Sa'd," because the larger star seems to swallow the smaller) ϵ, μ, ν Aquarii	23. Goei α Aquarii
24. Çravishthâ (Most glorious) $\beta, \alpha, \gamma, \delta$ Delphini	24. Sa'd as-Sûûd ("The luck of lucks" = specially lucky star β, ξ Aquarii	24. Che α Pegasi
25. Çatabhishaj (?) λ Aquarii, &c.	25. Sa'd al-Akhbiyah ("Sa'd with the tents") $\alpha, \gamma, \zeta, \eta$ Aquarii	25. Pi γ Pegasi
26. Pûrva-Bhâdrapadâ (Having ox feet) α, β Pegasi	26. al-Fargh al-Mukdim (The front lip of the bucket) α, β Pegasi	26. Koei ζ Andromedæ
27. Uttara-Bhâdrapadâ (Having ox feet) γ Pegasi, α Andromedæ	27. al-Fargh al-Mukhir (The hinder lip of the bucket) γ Pegasi, α Andromedæ	27. Leu β Arietis
28. Revatî (The rich) ζ Piscium, &c.	28. Baṭn al-Hût (The fish's belly) β Andromedæ, &c.	28. Oei 35 Arietis

I mention these, because although their dates are uncertain, they are undoubtedly built upon a common model, they have identical functions, and they have to do with the ecliptic, that is to say, we are in each case in presence of a belt of stars to which the motions of any other heavenly body travelling round the sun or, like the planets, round the earth, like the moon can be readily referred. In these lists[1] I give translations of the Sanscrit, Arab, and Chinese names, so far as they can be made out, and I must here express my deep obligations to Profs. Max Müller, Robertson Smith, and Douglas, for their kindness in supplying them.

J. NORMAN LOCKYER.

THE SECONDARY EDUCATION MOVEMENT.

THE outcome of the Oxford Conference on Secondary Education in England is our usual panacea for social ills, a Royal Commission. As this is to be, let us hope that the reference will be restricted to some definite points, and the members to a small number of properly qualified persons. Otherwise little else than unnecessary delay will be the result. Practical experience of such Commissions tends, however, to disenchant one with the prevailing idea of their usefulness, that is, of their power to settle the question at issue. Look at the last Commission on Primary Education, containing bigwigs of every kind. How long they sat, and how many Blue Books they filled with evidence, may be learnt by those who are interested. But what did it all come to? The large majority reported that they were totally opposed to free education, and the small minority, though not opposed, saw no possibility of its accomplishment. Two years afterwards a Tory Government carried a Free Education Act! Again, a Royal Commission on Vaccination has been sitting every Wednesday for the last five years, and it has not yet finished taking evidence! In face of facts like these, and they might be greatly extended, can one look with much hope to the early settlement of so difficult and complex a question as English secondary education by a Royal Commission as usually constituted? There are two conditions under which Commissions of this kind can act usefully: first, as means of inquiry into facts, and such a one was the Technical Commission of 1881–4, which journeyed over sea and land in quest of information; and second, as a means of carrying out measures laid down by Act of Parliament; and such a one, for example, is the Scottish University Commission now sitting. If we do not now know what we want in the way of secondary education, let there be a Commission by all means. Many may think that we do know. We are all convinced that more good secondary schools are needed both in town and country; and what has to be decided are such matters as how these schools are to be governed, by whom new ones are to be set up, and old ones remodelled to suit the wants of the times, how the necessary funds are to be found, and so forth. Now, are these questions of a kind which a Royal Commission can once for all determine? I think not. In my opinion they can only be settled by the House of Commons. The rival claims of County Councils, now in possession of the funds; of School Boards, now entrusted with primary education; of existing public schools of various orders; and, lastly, of private venture schools of all sorts and sizes, cannot be met or satisfied by any report of a Royal Commission. They must be fought out on the floor of the House, and it is by no means clear that the outcome of such a struggle will be in accordance with the recommendations which the report may contain. Therefore, desiring, as all those interested in education must do, to see the present chaos reduced to some degree of order without delay, and the crying needs at least to some extent supplied; and, knowing that there is no present prospect of Government action on such a scale as to systematise our varied forms of educational activity, I, for one, should be satisfied to get a Bill through the House of Commons consisting of two clauses, the first making the educational use of the whisky money compulsory and permanent, and the second giving County Councils power to expend such a portion as they think fit, of the funds capable of devotion to technical instruction, on the furtherance of secondary education. That an expenditure in this direction of some of the money especially voted by Parliament for technical instruction is justified by the acknowledged fact that it is impossible to carry on such instruction, except on the lowest level, to persons ignorant of the educational tools which have to be used.

But, in fact, technical instruction, as defined in the Act, is, or may be, modern secondary education, for it includes all the necessary subjects with the exception of classics and, perhaps, of English and history. So that under these Acts schools—either free or fee'd—can now be established wherever the County Councils determine, and these may be, to all intents and purposes, middle-class secondary schools. Moreover, under these Acts, the local authority may aid existing schools so as to enable them to give scientific or technical instruction. Both of these modes of action are now being widely adopted by County Councils all over the country, so that something substantial in the direction of what is needed is being done. There is, of course, considerable difference of opinion as to the best steps to be taken to bring about a complete and satisfactory system; and for the purpose of ventilating the subject, the Oxford University, in its corporate capacity, took the unprecedented step of calling a conference of the teachers of England, from the university to the elementary school, to discuss the whole question of secondary education. The gathering was remarkable in many ways, but chiefly as an admission, on the part of the universities, of the need of radical educational reform, and of the wisdom of their participation in such reforms. The papers read and the discussions held were, of course, of the multifarious and somewhat discursive order. All, however, from Dean Gregory on the one side, to Mr. Lyulph Stanley on the other, agree "that something should be done," but we may seek in vain for any consensus of opinion as to how that "something" is to be done, or even what that "something" is, except, indeed, the consignment of the matter to the tender mercies of a Royal Commission. Nevertheless, much good may come from the conference; many wise things were said, and the coming together of a large number of persons all in one way or another interested in assisting the progress of the question, cannot be without its useful effect.

What one misses chiefly in the discussion is the scientific aspect of the question. Scarcely a speaker touched upon what, I take it, is after all the gist of the whole matter, viz. the necessity, above all and under all, for an education based upon science. We have to deal, as was well said by Dr. Hewitt, of the Cheshire County Council, not with the 10 per cent. of the population to whom we teach the "humanities," but with the 90 per cent. of humanity struggling for existence. If we want to hold our own with foreign nations, we must alter, and that rapidly, and not waste our precious time too much in inquiry. With the object of raising the standard of existence for these teeming millions the nation now pays £750,000 a year, not enough, perhaps, to accomplish all we require, but amply sufficient for pre-

[1] Reproduced from the Journal of the American Oriental Society, vol. vi. No. II. p. 468, as given by Profs. Whitney and A. H. Newton.

sent use, especially when we remember that County Councils can, if they please, levy a rate in addition to the Imperial grant. We shall agree with Dr. Gull, of the Grocers Company's School, that the battle of scientific and technical training *versus* the humanities ought to be decided by evolution and natural selection, rather than by authority; though what this latter exactly means I do not quite understand. Authority can only act when evolution and natural selection—in other words, public opinion—has decided what is wanted. But we shall disagree with him when he says that "no time could be worse than the present for settling this question." We say that no time can be better, or rather that no time can be so good as the present; for if we do not settle the question soon it will be too late, and our people will lag so far behind those of other countries, that we shall not be able to fetch up our lee way, and the victory will not be to us. To my mind much nonsense is talked, especially by those whom I may without offence call the high-faluting educationists, about so-called culture, and of the necessity for the study of grammar and the humanities for children of every degree. Mr. Bowden, President of the Union of Teachers, is not one of these. He calls attention to the fact—deplorable enough—that only about five per cent. of our five million of children on the registers of elementary schools are in the sixth standard. This being so, it is our duty to give these few children whose parents are willing and able to let them pass on to a higher level of education in secondary schools, that which will most effectively fit them for the life which they afterwards have to follow. Ours is essentially an industrial population, and as the Duke of Devonshire said, at another conference on the same subject, "any advance in the direction of utilising existing secondary schools must be made not for the benefit of the middle classes only, but also of the whole of the working-class community of this country"; and to this I may add Mr. McCarthy's axiom, that school machinery which makes for clerkly employment at the expense of the skilled handicrafts, is so far harmful. Still, it is mainly our middle-class education which is in chaos and needs reform. The higher secondary education is probably sufficiently provided for by over 100 so-called "public" schools having a total of from 26,000 to 27,000 pupils. Primary education is under State direction, and will improve from year to year. To amend the middle-class education is more important even than to improve the educational ladder. Mr. Llewellyn Smith's most excellent report on the condition of secondary education in London shows how crying is the necessity in the metropolis for such middle-class schools. The few that exist in the kingdom are often insufficiently endowed, and their work is generally hampered by competition with private venture schools; and how bad the education is, which is given in many of these middle-class private schools, can, as Dr. Gull says, hardly be conceived. These inefficient schools must be either mended or ended before we can make much progress, and for this we need a Registration of Teachers Act, and an effective system of inspection. Honest private schools would benefit, and the others would disappear.

Of all the communications made concerning the relations present and prospective of the universities to the secondary education of the country, the letters read by Prof. Jebb, from the late Master of Balliol, are of the greatest interest, as giving the latest opinions of one who throughout his life was an educational, and especially a university, reformer. Dr. Jowett stated his desire that there should be a universal abiturienten examination, giving the right of admission to the universities. Then he wished to give all students who pass such an examination the right of becoming candidates, without residence or restriction of age, for any university examination, with or without honours, or for any part of the examination. He further remarks that such persons should have the privilege of admission to the libraries, of competing for university certificates and prizes without restriction of age. Moreover, he would give to such candidates as have shown any considerable merit, sums of money to enable them to carry on their inquiries; and this, says Prof. Jebb, was intended by Dr. Jowett to apply to all branches of knowledge without distinction, which the universities could best teach. These are indeed truly radical proposals, for they mean throwing open the university honours and emoluments to the world. That such measures should have been suggested in the almost dying words of the greatest master of the greatest of Oxford colleges, is in itself remarkable evidence of the present position of Oxford opinion. If fifty, or perhaps twenty, years ago, a radical undergraduate were to have made such suggestions, he would have stood a chance of being expelled from the university, like Shelley, for blasphemy; now they are the last words of Jowett, quoted in the presence of the Vice-Chancellor, with approval by Jebb.

H. E. ROSCOE.

THE SONNBLICK MOUNTAIN OBSERVATORY.

THE progress of meteorological science having rendered necessary a more careful investigation of the conditions of the higher strata of the atmosphere, the subject of mountain stations was considered at the Meteorological Congress at Rome, in 1879, and the various problems which could best be solved by such observations or in balloons were discussed. Among these may be mentioned:—The decrease of temperature with height, especially during cyclones and anticyclones; terrestrial and solar radiation; the behaviour of barometric maxima and minima at the earth's surface and at great heights, and the increase of wind velocity with height. Several important stations were already in existence, and the establishment of others was strongly recommended. Herr Ignaz Rojacher, the proprietor of the Rauris gold mines, having proposed to the Committee of the German and Austrian Alpine Club, in the year 1884, the erection of a meteorological station at the Miners' House on the Sonnblick, in the province of Salzburg, situated at an elevation of 7550 feet, about half-way between Kolm-Saigurn and the summit of the Sonnblick, Dr. Hann, director of the Austrian Meteorological Service, gladly took advantage of the suggestion, and in December of that year the station was equipped by the Austrian Meteorological Institute. But it was soon found that the site was unfavourable for the purpose, and Herr Rojacher decided that the only suitable position would be the summit of the mountain. After surmounting many difficulties, the work was satisfactorily carried out in the early part of 1886. The Alpine Club undertook the expense of the erection of a wooden house, while the Austrian Meteorological Society provided the self-recording instruments and undertook the building of a substantial tower for the anemometer and the establishment of telephonic communication between the summit and Rauris, a distance of 15½ miles, and, further, to supply instruments to the base station at Kolm-Saigurn. The accompanying illustration shows the position of the Observatory on the peak of the mountain; it is situated at a height of about 10,150 feet, and is the highest station in Europe. The difficulties of dragging the materials for the construction of the Observatory over glacier and snow for a distance of about 900 yards can hardly be overrated. Each trip occupied from three to four hours, and it was at this stage of the work that the greatest assistance was given by Herr Rojacher and his men. His intimate knowledge of the conditions of the glacier and *névé*, obtained from thirty-five years' residence in the neighbourhood, enabled him to select a favourable

site and to carry out successfully the construction of the building. Dr. J. M. Pernter has given a graphic description of the difficulties of an ascent which he made in February 1888 (NATURE, vol. xlii. p. 273), during which the foremost guides sank to their hips at every step, despite their use of snowshoes. The maintenance of the station in winter was a matter of great difficulty; but it was materially facilitated by the fact that the Miners' House workmen were at hand for the conveyance of fuel and for carrying out any necessary repairs. But in the year 1888 Rojacher was compelled, from failing health, to sell the mine, and in 1889 operations were discontinued; he succumbed in January, 1891, and then Kolm was abandoned altogether. Under these circumstances, the difficulty of continuing the Sonnblick Observatory was increased. The observer could not remain alone on the summit, separated from all human communication by a difficult journey of several hours over the snow, and it became necessary to hire men specially to carry up the fuel. The Salzburg section of the Alpine Club gave up the use of the house on the Sonnblick, and their contribution was, to a great extent, withdrawn, so that the maintenance of the Observatory was jeopardised. It was under these conditions that the Sonnblick Society, whose first report for 1892 we received a short time ago, was formed for the purpose of aiding in the expense of continuing this most important station. The Society already numbers 280 members, and, in addition to several other contributions, receives considerable subventions from the Austrian Government and the Committee of the German and Austrian Alpine Club.

Since the establishment of the Observatory in 1886, several valuable discussions on the conditions of the atmosphere in the higher regions have emanated from the pen of Dr. Hann and others, and these have already materially modified the prevalent ideas relative to the nature and origin of storms. In the present report Dr. Hann gives a general account of the climate deduced from observations taken up to the present time. From this it is seen that in each winter the temperature has fallen below minus 22°, and in March, 1890, it fell to minus 30°·3. The warmest month is August with a mean temperature of 33°·6, and the coldest month is February, mean temperature 5°·5. The precipitation is mostly in the form of snow; even in the six summer months, May to October, fully 85 per cent. of the fall consists of snow; out of about 200 wet days in the year, rain only fell on 21 days, and then it was often a kind of sleet. The greatest rainfall measured in one day was 2·8 inches on September 1, 1890. The amount of cloud is perhaps of most importance to tourists; this is most prevalent in June and least in December, just the opposite to what obtains in the lowlands. The month of June has only a quarter of the possible amount of sunshine, while in December it has about half the possible amount. Thunderstorms are less frequent on the Sonnblick than in the lower regions, and generally are not so severe. The Observatory is protected by a properly erected lightning conductor, and contains a room suitable for anyone who may wish to carry on researches at a great altitude. This room is entirely reserved for scientific purposes; another apartment, capable of accommodating twenty persons, has been provided for ordinary visitors. The Sonnblick Society deserves the thanks of all meteorologists for carrying on the work in their Observatory. The establishment of the mountain station has led to the elucidation of many obscure problems, and still more important results can confidently be expected.

NOTES.

MLLE. KLUMPKE, who has just gained the degree of Doctor in Mathematical Sciences at the Sorbonne, is the first lady who has obtained that distinction. The full title of her thesis was "Contribution à l'étude des anneaux de Saturne," and the following is a translation from *La Nature* of the complimentary terms in which M. Darboux addressed the gifted authoress in granting her the degree:—"You have occupied yourself with one of the most interesting questions in astronomy. The great names of Galileo, Huyghens, Cassini, and Laplace, without speaking of those of my illustrious colleagues and friends, are connected with the history of each of the great advances in the attractive but difficult theory of the rings of Saturn. Your work is not a slight contribution to the subject, and it places you in an honourable position among the ladies who have devoted themselves to the study of mathematics. During last century Mlle. Marie Agnesi gave us a work on the differential and integral calculus. Since then Sophia Germain, as remarkable for her literary and philosophic talent as for her mathematical faculties, was held in esteem by the great geometers who honoured our country at the beginning of this century. And but a few years ago the Academy of Sciences, on the report of a commission in which I had the honour to take part, awarded one of its best prizes to Mdme. Kowalewska, placing her name by the side of those of Euler and Lagrange in the history of discoveries relating to the theory of the movement of a solid body around a fixed point. In your turn you have entered upon your career. We know that for some years you have devoted yourself with great zeal and success to investigations connected

with the star-chart. Your thesis, which you have prepared according to our course of higher mathematics, with an assiduity that we could not ignore, is the first that a lady has presented and successfully sustained before our Faculty to obtain the degree of Doctor of Mathematical Sciences. You have worked in a deserving manner, and the Faculty has unanimously decided to declare you worthy of the grade of Doctor."

M. MAREY has accepted the Presidency of the French Photographic Society, in succession to Dr. Janssen, who has retired after completing his full term of office—three years.

THE *British Medical Journal* says that a branch of the Pasteur Institute will be established at Algiers next year.

THE death is announced of Dr. E. Lellmann, Professor of Chemistry at Giessen University.

WE regret to announce that Prof. A. Sprenger, the celebrated orientalist, died on December 19, at the age of seventy-five.

MR. W. L. H. DUCKWORTH has been elected to a Natural Science Fellowship at Jesus College, Cambridge. The new Fellow took a First Class in both parts of the Natural Science Tripos (1892-93), attaining distinction in Human Anatomy and Anthropology, and has published several papers on points connected with these sciences. His election is creditable both to his College and to the rising School of Anthropology which Prof. Macalister has founded in Cambridge.

THE *University Correspondent* says that the late Mr. Alexander Low Bruce, son-in-law of Dr. Livingstone, the explorer, has left £3,000 for the purpose of founding a Chair of Public Health in the University of Edinburgh.

THE Franklin Institute has awarded the following John Scott Legacy Medals and Premiums:—Mr. J. B. Edson, for his invention of a pressure-recording gauge; Mr. N. W. Perry, for his system of series electric traction for railways; Mr. J. T. Wilkin, for a method and apparatus for generating cycloidal surfaces; Mr. W. F. C. Morsell, for an application of polarised light to the systematic study of colour and crystal patterns for design, and Mr. F. Shuman, for his machine and process for embedding wire-netting in glass.

THE report of the Meteorological Council for the year ending March 31, 1893, which has recently been issued, contains an account of the progress in the various discussions in hand. In the branch of ocean meteorology, much valuable information has been added to the current charts for the Atlantic, Pacific, and Indian oceans, from the log-books of H.M. ships, and from data furnished by Foreign Governments. The latter observations are mostly for the Pacific Ocean, where they are comparatively scarce. For another investigation, the district between the Cape of Good Hope and New Zealand, all available data have been dealt with in the construction of monthly charts of the various elements, and the council have decided that the next district to be discussed shall be the South Atlantic. The work in the branch of weather telegraphy and forecasts continues to increase; comparisons of the results of forecasts issued during the hay harvest season, and of those regularly issued at night for the morning newspapers, show respectively a success of 88 and 79 per cent., taking an average of all districts, but for some localities the success was considerably higher. The work included under the climatology of the British Isles is steadily continued, and among the various investigations may be mentioned a discussion of the results of the harmonic analysis of the daily curves for temperature at the observatories, which has been laid before the Royal Society by General R. Strachey, chairman of the council, and of which an abstract is given in the report. Among the miscellaneous subjects we notice a description of a proposed new form of pressure gauge, which is the outcome of the investigations on wind measurements that have been carried out by Mr. W. H. Dines; and, also, that the council have commenced the regular tabulation of the hourly values of sunshine for seven observatories, since the year 1881. The results of an inquiry into fog observations, by Mr. Scott, for the years 1875-90, have been published by the Royal Meteorological Society.

AT the meeting of the French Meterological Society on December 5, M. Angot stated that while investigating the diurnal range of the amount of cloud at Paris, and representing it by a harmonic series, he had found that the semi-diurna period showed a range absolutely opposite to that of the diurnal variation of the barometer, so that the maximum of the semi-diurnal period in the amount of cloud corresponded to the minimum of pressure, and *vice versa*. We agree with M. Angot that it would be interesting to find whether the same relation holds for other places, in which case a proof would be given of the influence of the diurnal variation of the barometer upon the amount of cloud.

A DIFFERENTIAL method of determining the refractive index of solutions to which the interferential refractometer is not adapted, is described by W. Hallwachs in the current number of *Wiedemann's Annalen*. The former instrument is only applicable to measurements of differences of refractive power between very dilute solutions and their solvents. For higher concentrations the prism method has been generally used. But between these degrees of concentration and the former there lies a long series of solutions to which neither method is well suited. For these, Mr. Hallwachs has adopted the following arrangement. A beam of sodium light falls upon a plane-parallel plate of glass at an incidence of nearly a right angle, and is refracted into the glass and out on the other side, thus only skimming the surface on one side. The glass plate divides the solution from the solvent, and stands at right angles to another vertical plane-parallel plate, the two forming a combination in the form of a T. The beam traverses the solution and the second plate, and emergence at an angle with the first plate, which is read by a scale and micrometer. For differences of refractive index of 0·001, 0·005, and 0·1 the angles were in one case 6°, 13°, and 63° respectively, where with a prism of 60° they would only have been 0·15°, 0·8°, and 16°. In practice, the beam was first sent from the medium into the solution, and then from the solution into the medium, thus eliminating any errors in the position of the plates. The difference of refractive index was equal to the square of the sine of the angle of emergence divided by the sum of the two refractive indices. It was found that the molecular refractive index of the substance is decidedly affected by changes in concentration, accompanied by changes of molecular volume, while the specific refractive power of the substances is, curiously enough, very little disturbed. It appears that the effects of dissociation chiefly influence the density.

FOR some weeks there have been appearing in the *Electrician* accounts of devices for compensating the effects of hysteresis of the iron which is used in measuring instruments. The current number of that journal contains a short account, by Messrs. Field and Walker, of some experiments they have made, at Prof. Perry's suggestion, with this object in view. The principle of the method employed is to place a piece of hard magnet steel as a shunt between the poles of the electromagnet used in the instrument. When the magnetised current is passing, some of the lines of force pass through this shunt. When the current is broken, the lines of force in the air gap, due to the residual magnetism in the electromagnet and in the hard steel, will be in opposite directions, and by suitably proportioning the

length and section of the steel, it is possible to arrange that the two residual magnetisms shall just neutralise each others effect in the air gap. A rather novel method was employed to measure the current. A small rectangular tunnel was made in an ebonite plate, placed between the pole-pieces of the electro-magnet, connecting two large flat cylindrical vessels, one of which communicated with an almost horizontal glass tube. Electrodes were fixed on opposite sides of the tunnel, at the part of the field it was required to measure, so that a current could be passed across the tunnel. Under these circumstances, when there was a magnetic field between the pole-pieces, on passing a current between these electrodes and through the mercury, the mercury tended to flow from one of the cylinders to the other. A little alcohol placed above the mercury in the one cylinder flows along the inclined tube, and by its position indicates the pressure exerted by the mercury. The current through the mercury being constant, the pressure is proportional to the strength of the field. The authors say that by the above means it is possible to obtain visible readings for minute variations of the field, whether it be strong or weak.

IN the Proceedings of the Boston Society of Natural History, vol. xxvi. July, 1893, we note that Mr. Warren Upham reasserts his theory of the formation of drumlins in the neighbourhood of Boston. He now brings forward as confirmatory evidence the occurrence of deflected glacial striæ in Somerville, north-west of Boston. Mr. Upham's theory, which was fully expressed a year ago (Proc. B.S.N.H. vol. xxvi. December, 1892), supposes a rapid accumulation of the drumlins taking place during a period of rapid melting of the ice-sheet. Such periods were episodal occurrences, followed temporarily by re-advance of the ice or cessation of its melting; they mark the oscillatory nature of the influences which brought about the final recession of the ice from these areas. The material forming the drumlins was primarily collected by the ground-ice carried upwards through the ice-sheet as a result of the differential velocities of flow in the superficial and ground-ice. It became, after tracing a steep parabolic curve, part of the super-glacial drift, and began then a slowly descending movement, owing to surface-melting during the retreat of the ice. As the superficial drift was washed onwards and downwards over the melting ice, it was gradually transformed into true till. If now an advance of the ice-sheet took place, the super-glacial and englacial drift would be caught between a more rapidly-moving upper current of ice and a thin lower stratum of slowly-moving ice. The drift would suffer, therefore, from shearing movements, and be gathered into great lenticular masses, or sometimes long ridges, of drumlins, probably farther altered and added to before their final exposure. These are the main arguments of Mr. Upham's theory, to several of which grave objections have already been made by Prof. Davis, Mr. G. H. Barton, and others.

IN *Petermann's Mitteilungen*, November, 1893, a coloured map is published in illustration of Sir Thomas Elder's Australian Expedition of 1891-92. The map is reduced from the original to scale 1 : 3,000,000. The routes of travel, the stations at which the various observations were taken, and the geographical features of the country traversed, are all fully entered. The editor gives a brief statement of the objects of the expedition and its attendant success (pp. 269-270).

A WRITER in "Insect Life" (vol. vi. No. 1.), issued by the U.S. Department of Agriculture, describes a remarkable example of mimicry by a spider. At Jamesburg, N.J., in August of this year, his attention was drawn to what was apparently a gall, perfectly formed, and growing upon the upper surface of a leaf of a small oak tree. On handling the leaf, however, the supposed gall rolled off, and when it was picked up was found to be in reality a spider (*Ordgarius Cornigerus*, Hentz) which had been resting on the leaf, its curiously formed abdomen simulating exactly both in form and colour the common oak gall, even to the tiny punctures through which the gall insect makes its exit when mature.

GUSTAV EISEN has commenced the description of the earth-worms of California; though a dry and rainless country for six months in the year, it still would seem to possess a worm fauna rich both in species and individuals. With the exception of two very imperfectly described species by Kinberg, no true earth-worms have been recorded from this part of the world. While reserving a detailed account for the Transactions of the Californian Academy, Mr. Eisen gives diagnoses of Deltania, a new genus near Microscolex, with three new species; *D. elegans* (pretty widely diffused, and the largest species of the genus, being from two to four inches in length); *D. troyeri* (from Golden Gate Park, San Francisco); and *D. benhami* (Alameda County). Argilophilus is a new genus near Plutellus; *A. ornatus*, n. sp., is the most common earth-worm of the region, aud *A. papillifer*, n. sp., is a more southern form. (*Zoe* for October, 1893, vol. iv. p. 248.)

THE current number of *Danckelman's Mittheilungen* contains an account of an exploring trip made to the upper regions of Togo, between the years 1890 and 1892, by Captain E. Kling and Dr. R. Büttner. Copious extracts from Captain Kling's diary of his journey are given, with sketches of the features, tattoo marks, and head-dresses of the various tribes of natives he met with; there is also figured a hobby-horse played with by the children, which was made from a strong stalk of millet (in the drawing it looks like a bamboo cane), the head of a horse with ears and bridle being carved out of the root. Very extensive collections of the natural history of the countries visited were made, and astonishingly complete details, considering the time that has elapsed since the specimens were received in Berlin, are given. The lists of the mammals, reptiles, and amphibia of the Togo district are by P. Matschie, the birds of the environs of Bismarckburg are named by Dr. Ant. Reichenow, the fish and crustacea by Dr. F. Hilgendorf, the mollusca by Prof. E. Martens, the insects by Drs. H. Stadelmann and F. Karsch and H. J. Kolbe, the worms by A. Collin. The collection of plants has been only in part worked out, about 560 flowering plants species of were found, and a small number of ferns, lichens, and fungi. In a series of thirteen plates made from photographs taken by Dr. R. Büttner, there are views of Bismarckburg, of the natives of Adeli, Bimbila, &c. With investigations like these in addition to those pursued by our own countrymen, the tropical parts of Africa will soon be better known than the northern regions of the continent which lie within the sight of Europe.

THE order of the Laminariaceæ is one of the most distinct and well defined among the Phæosporeæ, the members of this order of sea weeds are all of comparatively large size, while the species of Macrocystis reach a length, even surpassing that of some tropical "climbers," and those of Lessonia possess stems which in appearance resemble the trunks of some trees. Since the date (1848) of J. G. Agardh's "Species Algarum" no attempt has been made to classify the numerous genera of this order, but in a very interesting memoir on the classification and geographical distribution of the Laminariaceæ, by W. A. Setchell, which appears in a recent number of the Transactions of the Connecticut Academy (vol. ix. 1893), we find the order divided into three tribes: I. Laminariideæ, with (1) Laminarieæ, containing the three genera, Chorda, Saccorhiza, and Laminaria; (2) Agareæ, with Agarum, Thalassiophyllum, Costaria, Cymathære, and perhaps Arthrothamnus. II. Les-

soniideæ, with (1) Lessonieæ and the genera Dictyoneuron, Lessonia, Postelsia, and Nereocystis; (2) Macrocysteæ, with Macrocystis. III. Alariideæ, with (1) Alarieæ, containing Alaria and Pterygophora; (2) Eckloniææ with Ulopteryx, Ecklonia, and Eisenia; (3) Egregieæ with Egregia. A synopsis of these nineteen genera is given, and also a list of all the known species with localities. The Laminariaceæ are inhabitants of the colder waters of the globe, it is the summer temperature which seems to act as a limit to their distribution, as heat not cold is inimical to their growth, they can endure almost any degree of cold that occurs even in polar seas, but speedily die away where the waters are at all warm.

IN support of his theory that the toxic products elaborated by pathogenic bacteria partake of the nature of ferments, Dr. Uschinsky (*Centrlb. f. Bakt.* 1893) quotes some very interesting experiments made by Courmont and Doyon, published in two lectures given before the Société de Biologie in March and June last. These investigators, in their studies on tetanus-poison, point out that the toxic action of this material is not hastened by greatly increasing the quantity of toxine introduced into an animal. Thus 200 c.c. of the filtrate obtained from a tetanus-broth-culture were injected into the blood of one dog, and from 3-4 c.c. of the same filtrate into another dog; in both cases tetanus symptoms developed on the third day after the injection. These investigators also state that they were able to induce symptoms of tetanus in animals, by simply injecting some of the blood derived from an animal rendered tetanic as above, similar results being also obtained with muscle-extract. That the quantity of the toxine introduced into the system of an animal does not influence the ordinary period of incubation characteristic for each variety of animal, was confirmed by experiments made by Dr. Uschinsky on rabbits; for by the injection of from 40-50 times the usual quantity of tetanus-toxine into these animals, he was not able to hasten the appearance of the tetanus symptoms. On the other hand, he failed to confirm the remaining results obtained by Courmont and Doyon, for on injecting 6-7 c.c. of the blood of rabbits suffering from tetanus into guinea-pigs and frogs, no symptoms of tetanus made their appearance in these animals. It is possible, however, that the French investigators used larger quantities of tetanus blood for their inoculations, and that this may account for the divergence in the results obtained.

Two contributions have recently been made towards the ætiology of the particular form of nervous disease known as delirium acutum. Early in the year Prof. Bianchi and Dr. Piccinino published a paper in the Transactions of the R. Accad. Med.-chirurg. of Naples, entitled "Sull'origine infettiva di una forma di delirio acuto," and in the *Centralblatt f. Bakteriologie*, vol. xiv. No. 16, Dr. Rasori has described the results of his observations on the ætiology of a particular case of delirium acutum which passed into his hands in the Bologna lunatic asylum. The investigations were, however, carried out in Prof. Tizzoni's laboratory, and under his direction. The clinical aspect of the case suggested some source of infection as the primary cause of the disease, and at the autopsy some of the fluid underneath the dura mater was removed and inoculated into sterile bouillon and agar-agar respectively, and kept at 37° C. In both these media bacterial growths made their appearance, which were found to be due to a small bacillus, the microscopic and macroscopic description of which is shortly given. The next step was to ascertain if this organism was endowed with pathogenic properties, and for this purpose ½ c.c. of a pure culture in broth of the bacillus was introduced, with the usual rigid antiseptic precautions, under the dura mater of a healthy rabbit. This animal died in two days, and the bacillus was found in large numbers in the blood and in the marrow taken direct from the brain and spinal cord. Similar results were obtained when the organism was introduced subcutaneously or into the nasal membrane, the bacillus being detected in the nerve substance as well as in the blood of the animal. The organism was, therefore, possessed of virulent pathogenic properties, being able to exist and multiply in the body of the rabbit, and by elaborating some toxic substance to induce its death in from one and a half to six days, according to the point selected for inoculation. Dr. Rasori, in making this preliminary communication, reserves to himself the task of trying to trace out the special circumstances which favour this bacterial infection, and the manner in which it may gain access to the human subject.

AN excellent photograph of the late Prof. Tyndall appears in Part 52 of "The Cabinet Portrait Gallery," published by Messrs. Cassell & Co., and is accompanied by a sketch of his career.

IN a Supplementary Paper, just issued by the Royal Geographical Society, Messrs. D. G. Hogarth and J. A. R. Munro give an account of "Modern and Ancient Roads in Eastern Asia Minor."

WE have received the *Monthly Weather Review* for July, 1893, prepared under the direction of Mr. A. Pedler, F.R.S., meteorological reporter to the Government of India.

THE third volume of Prof. Blake's "Annals of British Geology," 1892 (Dulau and Co.), has appeared. In previous volumes an objectionable feature was the insertion of criticisms among the abstracts of papers, but in this Prof. Blake's opinions are collected together in the form of an introductory review. We are afraid that the following extract from the preface heralds the death of the "Annals": "On the reception accorded to the present volume depends the continuance of the publication; the experimental stage is ended. The co-operation, therefore, is invited of all those who desire a record of geological literature of any kind, for the 'Geological Record' having fallen through, and the 'Year Book of Science' being discontinued, if these 'Annals' cannot be put on a firm basis the only remaining hope would be that the Geological Society should undertake a 'Record' at their own expense, by the aid of paid recorders."

AT the November meeting of the Institution of Engineers and Shipbuilders in Scotland, Prof. J. H. Biles read a paper on "The Strength of Large Ships," in which he gave the results of a series of calculations undertaken with a view of determining the relative stresses upon ships of more than 400 feet in length. Capt. J. Bain gave an account of experiments made with six large screw steamers to test "The Effect of Reversing the Screw of a Steamship upon the Steering." His experiments lead him to the following conclusions:—(1) If the helm is put hard aport on board a screw steamer, with a right-handed propeller, going full speed, or nearly full speed, ahead, and at the same instant the engines are reversed full speed, her head (provided there are no disturbing influences present) will cant to port instead of to starboard. (2) If the helm is put, or rather allowed to run hard astarboard, the instant the engines are reversed full speed ahead her head will cant to starboard as if *on a pivot*. (3) If a steamer, with a right-handed screw, going full speed ahead, has another vessel close to on her starboard bow, and in trying to clear her the helm is put hard astarboard and the engines reversed full speed, a collision is almost certain. With regard to the *Victoria* disaster Capt. Bain says: Had both the vessels started from their respective divisions at, say, under half speed ahead, with the port screw reversed on board the *Victoria*, and the starboard screw reversed on board the *Camperdown*, while the other screw on each of them was

kept at full speed ahead, the collision would have been averted.

A FURTHER communication concerning the explosive metallic derivatives of acetylene is contributed to the *American Chemical Journal* by Dr. Keiser. He has previously shown that the compounds obtained by the action of acetylene upon ammoniacal solutions of cuprous chloride and silver nitrate possess the composition C_2Cu_2 and C_2Ag_2, and that they are to be regarded as substitution products of the hydrocarbon. The action of acetylene upon aqueous and alkaline solutions of mercuric salts has since been studied, and the results are now published. When acetylene acts upon silver nitrate, either in aqueous or ammoniacal solution, the same product, C_2Ag_2, appears to be produced. But in the case of mercuric salts the action appears to be essentially different in aqueous and alkaline solutions. When acetylene is led through a solution of mercuric chloride in water, a white granular precipitate is produced, which is not explosive after drying, and which does not dissolve in dilute acids with evolution of acetylene gas. This compound contains chlorine, and is represented by the formula $C_2(HgCl)_2$. If, however, acetylene is passed into an alkaline solution of a mercuric salt, such as Nessler's solution, mercuric iodide dissolved in potassium iodide with the addition of caustic potash, a white flocculent precipitate is obtained, which when dry is extremely explosive and dissolves in dilute hydrochloric acid with evolution of acetylene. It is analogous in all its properties to the silver and copper compounds, and is a metallic substitution product of the same type, C_2Hg. This compound can only be obtained in the pure state by the use of pure acetylene, such as that prepared by treating ethylene dibromide with alcoholic potash. Decomposition of the substance commences at about 100°, and when it is rapidly heated to a temperature slightly higher than this it explodes with extreme violence, leaving a small residue of finely divided carbon and mercury. It is particularly interesting to learn that when the substance is treated with an alcoholic solution of iodine it unites with the latter, slowly even at the ordinary temperature and rapidly at the temperature of a water-bath, forming di-iodo-acetylene, C_2I_2. This compound upon standing a short time polymerises, and the polymer separates in the form of crystals which melt at 187° and appear to be hex-iodo-benzene, C_6I_6.

IN the course of an investigation concerning the atomic weight of copper, Messrs. Richards and Rogers, of Harvard University, have observed that cupric oxide prepared by ignition of the nitrate always contains a considerable amount of occluded gas, chiefly nitrogen, while that prepared from the carbonate invariably shows no sign of occluded gas. This fact is of considerable importance inasmuch as the previous determinations of the atomic weight of copper are affected by it and will be rendered more or less inaccurate. It is no new observation, however, for it was pointed out by Frankland and Armstrong as long ago as 1868, but appears to have been largely overlooked. The Harvard chemists show, moreover, that the phenomenon is also exhibited by oxides of zinc, nickel, and magnesium, when prepared by ignition of the nitrate. In the case of magnesia the amount of occluded gas is extraordinarily large, exceeding a hundred cubic centimetres from ten grams of oxide. Hence it is considered necessary that the atomic weights of these metals should be subjected to revision, taking account of these facts, and until this is done the values hitherto accepted can only be considered as approximate.

THE additions to the Zoological Society's Gardens during the past week include a Mona Monkey (*Cercopithecus mona*, ♀) from West Africa, presented by Mrs. Frances Bell; a Bonnet Monkey (*Macacus sinicus*) from India, presented by Mr. Henry Vane; three Black-backed Jackals (*Canis mesomelas*) from South Africa, presented by Mr. J. Matcham; four Bernicle Geese (*Bernicla leucopsis*), 2 ♂, 2 ♀) European, a Variegated Sheldrake (*Tadorna variegata*) from New Zealand, presented by Sir Henry Peek, Bt.; a Little Auk (*Mergullus alle*) British, presented by Mr. J. W. C. Stares; two Adorned Ceratophrys (*Ceratophrys ornata*) from Buenos Ayres, presented by Miss Mildred FitzHugh; a Mozambique Monkey, (*Cercopithecus pygerythrus*) from East Africa, deposited; six Smooth-clawed Frogs (*Xenopus lævis*) hatched in the Gardens.

OUR ASTRONOMICAL COLUMN.

SMALL DISTANCES MEASURED WITH THE HELIOMETER.—As the filar micrometer measures very precisely distances of a few seconds, so with equal accuracy does that important of modern instruments, the heliometer, measure distances ranging from a few minutes up to one or two degrees. With the former instrument one brings the thread (moved by the micrometer head) first to one side of the star and then to the other; or, if one has two threads, one places the star half-way between them, and thus reads off the distance. With the heliometer the distances are read off the scale at the object-glass end (from the eye end), the images of the two stars cast by the movable half of the object-glass being placed symmetrically first on one, and then on the other side of the images from the fixed half.

In the measurement of small distances with the heliometer it has been found that nearly every observer measures the distances too small, thus a small positive correction has to be applied to the observations. As an instance of the magnitude of the corrections to be added or subtracted for various distances, we give the following table:—

Mean distance	CAPSTADT.			Yale Obs.	GÖTTINGEN.	
	Gill.	Finlay.	Jacoby.	Chase.	Schur.	Ambronn.
″	″	″	″	″	″	″
1000	+0·03	+0·07	+0·18	+0·14	+0·20	+0·14
2000	+0·01	0·00	+0·13	+0·08	+0·03	+0·02
3000	+0·01	−0·04	+0·13	+0·08	+0·09	+0·11
4000	+0·01	0·00	0·00	−0·01	−0·08	−0·11
5000	−0·06	−0·05	−0·15	−0·10	−0·01	−0·15
6000	−0·04	−0·13	−0·21	−0·18	−0·12	−0·22
7000	0·00	0·12	−0·12	−0·08	−0·07	−0·21

The necessity of such a correction has long been known, but its cause is yet unexplained, although suggestions, such as that of Dr. Gill, have been put forward. In *Astronomische Nachrichten*, No. 3197, Prof. Wilhelm Schur gives an interesting account of his investigations, which had for their aim the determination of the source of this error. Observations showed him that the explanation suggested some time back by Dr. Gill was not valid, at any rate for the Göttingen heliometer; it was finally thought that an explanation might be found by supposing an irregular guidance to occur to those parts of the instrument which carry the objective, but on investigation it was found that such was most improbable. Irregularity in the objective slides themselves was also eliminated, as the magnitude of the error necessary to produce such a large difference in the distance measured was too large to be at all considered. In the paper Prof. Schur is led to discuss some of the measurements made in the triangulation of the Præcipe, and he refers to the method he adopted to bring all the measures into harmony. Three ways were open to him to satisfactorily accommodate the distances in this work, they being, in the measurements of the three arcs, the employment of quantities which are too small to be of no account in the measures of distances in the large quadrilateral. Development of the formula on the assumption (as the observations indicate) that at the distance 1300″ a maximum in the correction occurs, and for 0″ and 5000″ the "verbesserung" is 0, and *a* is assumed to be 0″·473. And lastly, the computation of the "verbesserung" after the expression 0″·264 s., which assumes a change in the scale value, which change, although inadmissible for large distances, brings, in the case of the distances here concerned, the measurements into a more satisfactory concord.

THE TAIL OF COMET BROOKS (c. 1893).—The tail of this comet seems to have undergone some interesting changes, and the following brief descriptions from two well-known observers will show the different appearances observed. Mr. Brooks describes the tail on October 21, 17h, as having a sharp curve close to the head towards the south and accompanied by a faint secondary tail, issuing from the head at an angle of 30° to the main tail towards the north (*Astronomy and Astrophysics* for December). On November 4, the tail assumed its usual straight form, but on November 9, 17h, it was straight for a length of half a degree from the head, where it became forked, the larger portion curving gracefully to the south, the fainter part straight, or nearly so, branching to the north, the two branches making an angle with each other of about 25°. Prof. Barnard, who has obtained several pictures, found that they showed undoubtedly that on October 21 the tail had encountered some outside or obstructive medium which badly shattered it. Rapid and some very remarkable changes in position angle were also gathered from an examination of the plates. The advantage of photography for obtaining cometary photographs, and especially for making analyses of the tails, will be at once grasped when one considers that Prof. Barnard, with the 12 in., could not trace the tail even to a distance of 1°, while the photographs taken with the Willard lens (6 in. aperture, 31 in. focus) showed it fully for 10°.

HYDROGEN ENVELOPE OF THE STAR D.M. + 30°3639.—Prof. W. W. Campbell, in the December number of *Astronomy and Astrophysics*, communicates a very important observation with regard to the spectrum of one of the Wolf-Rayet stars. The star in question is of the 9·3 magnitude, D.M. + 30°3639, and its spectrum is very rich in bright lines. The most striking features of the visual spectrum have been noted as the bright line λ 5694, the bright blue band at λ 4652, and the very bright hydrogen line $H\beta$. By arranging the spectroscope so that each of these different parts of the spectrum is in focus, the line λ 5694 is seen as "a very small image of the star." The band at λ 4652 is "broad and lies wholly upon the narrow continuous spectrum," the $H\beta$ line observed with a narrow slit "is a long line extending to a very appreciable distance on each side of the continuous spectrum," and with an open slit is "a large circular disc 5″ in diameter." Other hydrogen lines $H\gamma$ and $H\alpha$ also exhibit the same peculiarities. The explanation of this appearance is that the star in question must be surrounded by an envelope of incandescent hydrogen, for other lines in the same spectrum are not so changed. It is remarked also that in other stars of the same type no such image has been observed.

"L'ASTRONOMIE" FOR DECEMBER.—The December number of this journal commences with a most interesting article by Dr. Janssen on the Observatory at Mont Blanc. The article itself contains nothing of which our readers have not been informed in the previous columns of NATURE unless it be the illustration showing the summit of the mountain with the observatory "in winter." Two good illustrations of the appearance of the sun during the last total eclipse of the sun (April 16, 1893), the clichés of which were obtained by M. Schœberle and Prof. Deslandres. "Around the world of Jupiter in ten hours" is the title of a series of observations made at the observatory of Juvisy by M. Eugène Antoniadi The writer gives twelve drawings of this planet, as made during this period, showing the various surface markings which were brought to view by rotation. Amateurs and others who at this time are observing this planet will find these drawings a most useful help in recognising many markings. The red spot is described as excessively pale: "Elle est colorée en rose; ses regions centrales sont claires, ses bords plus sombres; elle est entourée d'une auréole blanchâtre."

GEOGRAPHICAL NOTES.

THE death of Dr. H. Rink, on December 15, removes the greatest authority on Greenland and the Eskimo. His life-long devotion to the problems of the Arctic people gained for him the esteem of all geographers.

REUTER'S AGENCY announces that information has been received from Baron Toll to the effect that up to October 25 Dr. Nansen had not called at the Olenek river. This is practically decisive news that Dr. Nansen found the sea so open that he determined to push northward without delaying to call anywhere; and it is improbable that we shall hear more of the intrepid traveller until we receive his own report of success or failure.

DR. MURRAY'S paper on the renewal of Antarctic exploration will be published in the January number of the *Geographical Journal*, which commences the third volume. It will be accompanied by a series of letters from distinguished foreign oceanographers and naturalists, strongly urging the importance of resuming systematic exploration in Antarctic seas.

DR. HILLIER has recently communicated a paper to the Vienna Academy of Sciences on the geography of the Pindus range, one of the few mountain systems of any extent in Europe which has never yet been adequately explored. He finds that the system consists of three parallel ranges, and he has unravelled the geological structure of each.

A COMMUNICATION to the Royal Geographical Society states that Mr. Crawshay, a Government official in British Central Africa, has recently visited the Angoni country north-west of Lake Nyasa. He found the Nyika Plateau, which was traversed on the way, a magnificent country, inhabited by a scattered population of Anyika, living in huts built on narrow terraces on the mountain-side or in caves, and cultivating peas as an almost exclusive crop. In this district there are some fine mountains, exceeding 8,000 feet in height, the principal town of the Anyika, on the slope of Kantorongondo, being nearly 6,000 feet above the sea.

EPIDEMIC INFLUENZA.[1]

THE present report, a welcome supplement to the epoch-making report on the epidemic of 1889-90, is divided into eight parts, the first seven by Dr. Parsons, and the last one by Dr. Klein. It includes statistical studies of the epidemic of 1890, an account of the recent epidemics in England and Wales, a history of influenza abroad in 1891 and 1892, considerations respecting the ætiology of the disease, notes on some clinical features of the later epidemics, reports on outbreaks in institutions, &c., remarks on the prophylaxis of the disease, and, in Dr. Klein's department, a report on influenza in its clinical and pathological aspects, to which photographic plates are appended exhibiting influenza bacilli.

Among the conclusions confirmed by the present report are the small influence of locality, or environment, and the invariably potent factors of exposure or proximity to the sick, and bad ventilation. Over and over again serious epidemics in a town or island have been traced to the arrival of one or two persons from an infected place. With regard to the later epidemics, it would appear that the contagion of the disease, scattered broadcast, had "retained its vitality, but in a suspended or inconspicuous form, perhaps by transmission from one human being to another in a succession of mild sporadic cases, perhaps in some medium external to the human body." Recrudesence has taken place chiefly in early spring and in autumn. Observers in various parts of the world have contributed their experience that the progress of influenza in a country is gradual. The most remarkable instances of rapid and wide diffusion were in the United States, especially in the Western States and to settlements far apart.

A good example of the usual manner of spread is given in Part IV. A teacher of music visited two relatives ill with influenza on April 6, and returned to his own locality, which had been hitherto unaffected. On April 9 he was attacked, but struggled through his work, and gave lessons to pupils at several houses. On April 11, ten of his pupils, and on April 12, the people with whom he lodged, developed the disease.

One medical officer states that he recollects no instance of the disease spreading from one member of a household to others where strict precautions for isolation and disinfection were taken. Unfortunately, however, it often happens that the first member attacked was not the only one who had been previously exposed to infection. Dr. Newsholme, medical officer for Brighton, states that the borough sanatorium, being very strictly isolated in every respect, escaped during the first two outbreaks, and in the third until a servant who had been absent

[1] Further report and papers on Epidemic Influenza, 1889-92. With an introduction by the Medical Officer of the Local Government Board. 1893.

returned and fell ill with influenza; strict isolation was even then successful in preventing its spread to the inmates.

Dr. Caldwell Smith's evidence as to ætiology is valuable and interesting. "It is to the life history of Pfeiffer's bacillus that we must direct our attention if we wish to understand the seemingly strange vagaries of the disease. An individual is infected by breathing at once the expired air from a person suffering from the disease, and I believe this to be the only method of infection."

The concourse of people is favourable to the spread of influenza in two ways, according to Dr. Parsons: firstly, by bringing the affected and the healthy near together; and secondly, by the poison being present in a more concentrated form in confined and vitiated air.

Among the discussions which throw light on the character of the disease, and bear upon the means of prevention, the following may be mentioned: on the degree of protection afforded to individuals and to communities by previous attacks, on the influence of occupation and of unsanitary conditions, on the connection with pneumonia, on the period of infectiousness, on the clinical features of the later epidemics, and on relapses.

The researches of Dr. Klein, in respect to the effect of inoculation upon animals, gave results for the most part negative. His affirmative results, however, were "in full agreement with the results obtained by Pfeiffer and Kitasato." The bacillus was always abundantly present in the bronchial secretion of patients suffering from influenza, diminishing in number as the disease abated.

"It is to be feared," wrote Dr. Parsons, "that the contagion of influenza is still domiciled among us, and that a renewal of its epidemic activity within the next few years is by no means improbable." The expected revival is now only too apparent. A certain proverb declares, with the rashness of its class, that the man once bit is twice shy. In a literal sense, the saying may contain a good deal of truth, but to nations, or aggregations of individuals, it is quite inapplicable. The development of common sense for common action against these evils has still to take place. This country has now passed through three severe epidemics of influenza within four years, each outbreak drawing many sad maladies in its train, prostrating hundreds of thousands of breadwinners, cutting short many illustrious lives, and crippling many for years to come, and we are now running into a fourth epidemic in London, without any great organised attempt being made to counteract it.

The provisional memorandum of the Local Government Board, issued on January 23, 1892, impressed upon the public the fact that in its epidemic form influenza is an eminently infectious complaint, communicable in the ordinary personal relations of individuals with each other, that separation of the sick from the healthy should as far as practicable be carried out, that rooms, &c. should be disinfected, and that ventilation should receive special attention.

It would be some defence against a serious recrudescence of the pest if this memorandum, or an abstract of it, were supplied to every householder on the first threatenings of an outbreak in any locality. In his article on prophylaxis, Dr. Parsons remarks on the difficulties which would frustrate any measures of notification and isolation on a large scale, but suggests that notification, with fees for early cases only, might be tried in certain districts, and that such a measure should be adopted "in the interval before another epidemic." So much experience has been gained in distinguishing the symptoms of influenza from those of other ailments, that the difficulty of diagnosis cannot now be an insuperable bar to attempts at prevention. It is well to remember that the pecuniary cost of prevention cannot be compared with the loss to the country by an epidemic, for this has been proved to amount to millions.

Among places and means of infection which may cause much mischief, but are not noticed in this volume, are bakers' shops, in which the baker or attendant suffers from influenza or severe cold; booking offices, post offices, banks, &c. in which the mouth and the ledger, &c. are in multiple communication; letters written and fastened by patients; and, most of all, railway carriages packed full and with windows closed, daily conveying vast numbers of people to and from the city, and containing perhaps the most organically polluted air which can easily be found in a civilised country.

The report closes with an interesting statement respecting the immunity of animals, including monkeys, at the Zoological Gardens.

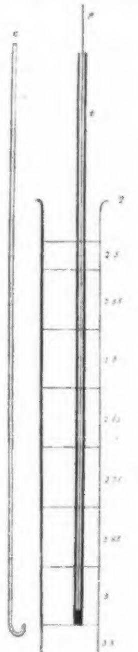
R. RUSSELL.

ON A METHOD OF SEPARATING THE MINERAL COMPONENTS OF A ROCK.

IT is told of a famous German petrographer, that whenever appealed to by a student in difficulties over a problematical mineral in a rock-slice, his invariable advice was "Get it out."

It is hard dispassionately to reflect on the sufferings to which this simple process of "getting it out" have given rise. All we petrographers have passed through the vale! May we now indulge the pious hope that the following simple apparatus may bring some mitigation to the ordeal? It will certainly save a good deal of time and trouble when only small quantities of a particular mineral are required; enough, that is, for a blow-pipe analysis, a flame test, and microscopical examination.

A large test-tube (see Fig.), conveniently six inches in length by three-quarters in diameter, is filled with heavy solution, graded from specific gravity 3·3 to 2·5, so as to form after standing a diffusion column, as already described in NATURE, vol. xliii. p. 404, 1891. It is not necessary to wait till the change in density of the column is uniform from top to bottom; by introducing a sufficient number of specific gravity indexes the column is mapped out into a succession of lengths, within the limits of each of which the change of density is practically uniform, certainly sufficiently so for mineral determinations.



A test-tube containing a diffusion column. The figures at the side indicate corresponding specific gravities: *t*, pipette fitted with a piston *p*; *c*, crook by which the lower end of the pipette may be plugged.

A fragment of the rock to be examined, about the size of a hazel-nut, is powdered in the usual way, sifted and washed, dried and then introduced into the diffusion column. Separation of the constituent minerals at once begins to take place, and in the course of a few hours is complete. Each species of mineral is then floating in liquid of its own specific gravity; the next problem is to get it out. A pipette as commonly used is not sufficient, for as it is introduced grains of minerals from other zones than that sought for, adhere to its sides; on removing the finger, the sudden inrush of fluid carries with it grains from surrounding zones, and finally on drawing up the pipette, fluids of zones lying above that to which it has descended displace the heavier fluid it already contains, carrying with them suspended grains, and thus bringing about the mixture which it is our desire to prevent.

With very little trouble these difficulties may be completely overcome. To prevent the sudden inrush of fluid the pipette, which should be of small calibre (in my experiments it measures 1·5mm.), is fitted with a piston (*p*). This may be very simply made by winding a little unravelled cotton thread round the end of a stem of Esparto grass, such as is sold for cleaning tobacco-pipes. The piston is pushed down to the bottom of the pipette, which is then ready for use.

To extract grains from any zone the pipette is slid down into the diffusion column till its lower end is just immersed in the zone; a gentle shake given to it as it passes through the solution will serve to detach adhering particles; the piston is then slowly raised, and the fluid with its floating mineral grains quietly follows it, the other zones remaining undisturbed. To prevent the fluid of higher zones entering the pipette as it is withdrawn, it is necessary to plug its lower end; no very tight closure is necessary, since the piston, which now lies at the upper end of the pipette, by excluding the air ensures the retention of the contained column of fluid; all that is needed is a stopper, which will exclude solid particles. A very thin glass rod is rounded off at one end, which is then bent upwards into the form of a crook (*c*). The crook is let down into the diffusion column till its upward pointing lower end lies beneath the open extremity of the pipette, which it completely blocks up on being

raised into position. The pipette with the crook is then taken out of the fluid, and inverted. The crook is laid aside, and the outside of the pipette cleaned with blotting-paper, by which all adhering foreign grains are removed. The pipette now contains a pure gathering of the mineral required, and it only remains to discharge its contents, and this is of course accomplished by pushing down the piston.

Minerals may thus be removed from every zone of a diffusion column; and all the species which enter into the composition of a rock, except of course the very heaviest, may be separately obtained, with their specific gravity determined as an incident of the process.

In this absurdly simple fashion "getting it out" ceases to be a penance, and becomes a pleasure. W. J. SOLLAS.

THE CLOUDY CONDENSATION OF STEAM.[1]

THE air, as every one knows, is composed almost entirely of the two gases, oxygen and nitrogen. It also contains small quantities of other substances, of which the chief are carbonic acid gas and water vapour, and it is the latter of these constituents, water vapour, or "steam," as it is sometimes called, that will principally concern us this evening.

The quantity of invisible water vapour which the air can at any time take up depends upon the temperature; the higher the temperature of the air the more water it can contain. The proportion. however, never exceeds a few grains' weight of water to a cubic foot of air. Air at any temperature, containing as much water as it can possibly hold, is said to be "saturated," while the temperature at which air containing a certain proportion of water becomes saturated is called the "dew point."

The large glass globe, upon which the beam from the electric lantern is now directed, contains ordinary air, kept in a state of saturation, or nearly so, by the presence of a little water. You will observe that although heavily laden with water vapour the air is perfectly transparent. If, now, we turn a tap, and so connect the globe with an exhausted receiver, the air expands and becomes colder; the space inside the globe is no longer able to hold the same quantity of water as before in the form of vapour, and the excess is precipitated as very finely divided liquid water, which fills the globe and is perfectly visible as a cloud or mist. In a few minutes the cloud disappears, partly, no doubt, because some of the particles of water have fallen to the bottom of the vessel, but chiefly because the air becomes in time warmed up to its original temperature (that of the room), and the suspended water is converted back again into invisible vapour.

I once more rarefy the air, and admit a fresh supply while holding the flame of a spirit lamp near the orifice of the inlet pipe, so that some of the burnt air is carried into the interior of the globe. When the air is again expanded a cloud is formed which is far more dense than the others were. It appears on examination that the increased density of this cloud is not due to the condensation of a greater quantity of water. Little, if any, more water is precipitated than before. But the water particles are now much more numerous, their increased number being compensated for by diminished size. Within certain limits, the greater the number of particles into which a given quantity of water is condensed, the greater will be the apparent thickness of the mist produced. A few large drops will not impede and scatter light to the same extent as a great number of small ones, though the actual quantity of condensed water may be the same in each case.

Then comes the question, why should the burnt air from the flame so greatly increase the number of the condensed drops? An answer, though perhaps not quite a complete one, is furnished by some remarkable experiments made by M. Coulier, a French pro fessor, nearly twenty years ago. He believed his experiments showed that water vapour would not condense at all, even at temperatures far below the dew point, unless there were present in the air a number of material particles to serve as nuclei around which the condensation would take place. All air, he says, contains dust; and anything that increases the number of dust particles in the air increases the density of the condensation by affording a greater number of nuclei. Air in which a flame had been burnt he supposed to be very highly charged with finely-divided matter, the products of combustion, and thus rendered extraordinarily "active" in bringing about condensation. And that, according to Coulier's view, is the reason why such a dense fog was formed when air which had been contaminated by the spirit flame was admitted to our globe.

[1] Extracted from a lecture on "Fogs, Clouds, and Lightning," delivered at the Royal Institution on May 5.

On the other hand, air, even burnt air, which has been filtered through tightly packed cotton wool, is found to be perfectly inactive. No cloud or mist will form in it, however highly it may be supersaturated. Coulier explained this fact by supposing that the process of filtration completely removed all dust particles from the air.

The experiments of Coulier were repeated and confirmed by Mascart. The latter also made one additional observation which may very probably turn out to be of great importance. He found that ozone, or rather, strongly ozonised air, was a very active mist producer, and that unlike ordinary air, it was not deprived of its activity by filtration.

Four or five years later, all the facts which had been noticed by Coulier, and others of an allied nature, were independently discovered by Mr. Aitken, who has devoted much time and study to them, and made them a foundation of an entirely new branch of meteorology.

Later, perhaps, we may see reason to doubt whether all the conclusions of Coulier and Aitken are quite accurate, especially as regards the action of so-called products of combustion.

Every one has noticed how dense and dark a thundercloud is. It shuts out daylight almost as if it were a solid substance, and the glimmer that penetrates it is often imbued with a lurid or copper-coloured tint.

I had always found it rather difficult to believe that these peculiarities were due simply to the unusual extent and thickness of the clouds, as is commonly supposed to be the case, and it occurred to me about three years ago, that perhaps some clue to the explanation might be afforded by the electrification of a jet of steam. On making the experiment I found that the density and opacity of the jet were greatly increased when an electrical discharge was directed upon it, while its shadow, if cast upon a white screen by a sufficiently strong light, was of a decidedly reddish-brown tint.

As a possible explanation of the effect I suggested that there might occur some action among the little particles of water of a similar nature to that observed by Lord Rayleigh in his experiments upon water jets. A jet of water two or three feet long is made to issue in a nearly vertical direction from a small nozzle. At a certain distance above the nozzle the continuous stream is found to break up into separate drops, which collide with one another, and again rebounding, become scattered over a considerable space. But when the jet is exposed to the influence of an electrified substance, such as a rubbed stick of sealing-wax, the drops no longer rebound after collision, but coalesce, and the entire stream of water, both ascending and descending, becomes nearly continuous.

There is one other point to which I wish to direct your particular attention. If the sealing-wax, or better, the knob of a charged Leyden jar, is held very close to the jet, so that the electrical influence is stronger, the separate drops do not coalesce as before, but become scattered even more widely than when no electrical influence was operating. They become similarly electrified and, in accordance with the well-known law, repel one another.

We will now remove the water jet, and in its place put a little apparatus for producing a jet of steam. It consists of a half-pint tin bottle, through the cork of which passes a glass tube terminating in a nozzle. When the water in the bottle is made to boil, a jet of steam issues from the nozzle, and if we observe the shadow of the steam jet upon the screen we shall see that it is of feeble intensity and of a neutral tint, unaccompanied by any trace of decided colour. A bundle of needles connected by a wire with the electrical machine is placed near the base of the jet, and when the machine is worked electricity is discharged into the steam. A very striking effect instantly follows. The cloud of condensed steam is rendered dense and dark, its shadow at the same time assuming the suggestive yellowish-brown colour.

I at first believed that we had here a repetition, upon a smaller scale, of the phenomenon which occurs in the water jet. The little particles of condensed water must frequently come into collision with one another, and it seemed natural to suppose that, like Lord Rayleigh's larger particles, they rebounded under ordinary circumstances, and coalesced when under the influence

of electricity. The great majority of the small particles ordinarily formed consisted, I thought, of perhaps only a few molecules, which were dispersed in the air, and again converted into vapour without ever having become visible, while the larger particles formed by their coalescence under electrical action were of such dimensions as to impede the more refrangible waves of light. Hence the brownish-yellow colour.

Other explanations have been proposed. There is the molecular shock theory of the late R. Helmholtz (who, as it turned out, had studied electrified steam jets before I made my own experiments). I shall refer to his speculation later. And there is the dust-nucleus theory, which no doubt appears a very obvious one.

Though I knew that my own hypothesis was not quite free from objection, neither of these alternative ones commended itself to me as preferable; and so the matter rested until a few months ago, when the steam jet phenomenon was discussed anew in a paper communicated to the Royal Society by Mr. Aitken. Mr. Aitken said that he did not agree with my conjecture as to the nature of the effect. This led me to investigate the matter again, and to make some further experiments, the results of which have convinced me that I was clearly in error. At the same time it seems to me that the explanation which Mr. Aitken puts forward is little less controvertible than my own. Mr. Aitken's explanation of the phenomenon is, like mine, based upon Lord Rayleigh's work in connection with water-jets, but, unlike mine, it depends upon the experiment which shows that water particles when strongly electrified are scattered even more widely than when unelectrified. He believes, in short, that electrification produces the effect, not by promoting coalescence of small water particles, but by preventing such coalescence as would naturally occur in the absence of electrical influence. In the electrified jet, he says, the water particles are smaller, but at the same time more numerous; thus its apparent density is increased.

The chief flaw in my hypothesis lies in the fact that the mere presence of an electrified body like a rubbed stick of sealing-wax, which is quite sufficient to cause coalescence of the drops in the water jet, has no action whatever upon the condensation of the steam jet. There must be an actual *discharge* of electricity. But it is by no means essential, as Mr. Aitken assumes, that this discharge should be of such a nature as to electrify, positively or negatively, the particles of water in the jet. If, instead of using a single electrode, we employ two, one positive and the other negative, and let them spark into each other across the jet, dense condensation at once occurs. So it does if the two discharging points are removed quite outside the jet. A small induction coil giving sparks an eighth of an inch in length causes dense condensation when the electrodes are more than an inch distant from the nozzle and on the same level. In one experiment a brass tube two feet long was fixed in an inclined position with its upper end near the steam jet, and its lower end above the electrodes of the induction coil. In about three seconds after the spark was started dense condensation ensued, and it ceased about three seconds after the sparking was stopped. No test was needed, though in point of fact one was made, to show that the steam was not electrified to a potential of a single volt by this operation. And the time required for the influence to take effect showed that whatever this influence might be it was not induction.

The inference clearly is that in some way or other the action is brought about by the air in which an electrical discharge has taken place, and not directly by the electricity itself. The idea has no doubt already occurred to many of you that it is a dust effect. Minute particles of matter may be torn off the electrodes by the discharge, and form nuclei upon which the steam may condense. The experiments of Liveing and Dewar have indeed shown that small particles are certainly thrown off by electrical discharge, and the idea that such particles promote condensation appears to be supported by the fact that if a piece of burning material, such as touch-paper, is held near the jet so that the products of combustion can pass into it, thick condensation is produced.

From a recent paper by Prof. Barus, published in the *American Meteorological Journal* for March, it appears that he also is of opinion that such condensation is in all cases due to the action of minute dust particles. Yet it is remarkable that Mr. Aitken, the high priest and chief apostle of the philosophy of dust, gives no countenance to the nucleus theory. He does not even advert to its possibility. I imagine that his experiments have led him, as mine have led me, to the conclusion that it is untenable. And this not only in the case of electrical discharge, but also in the case of burning matter.

If we cause an electrical discharge to take place for some minutes inside a suitably arranged glass bottle, and then, ten or fifteen seconds after the discharge has ceased, blow the air from the bottle into the steam jet, the condensation is not in any way affected. Yet the dust could not have subsided in that time. And again, if we fill another large bottle with dense clouds of smoke by holding a bundle of burning touch-paper inside it, and almost immediately after the touch-paper is withdrawn, force out the smoke-laden air, through a nozzle, upon the jet—you can all see the black shadow of the smoke upon the screen—nothing whatever happens to the jet. Yet a mere scrap of the paper which is actually burning, though the ignited portion may not be larger than a pin's head, at once darkens the jet. Dead smoke (if I may use the term) exerts little or no influence by itself: there must be incandescent matter behind it. The question naturally arises, whether incandescent matter may not be sufficient of itself, without any smoke at all. We can test this by making a piece of platinum wire red-hot and then holding it near the jet. It is seen to be quite as effective as the burning touch-paper. Yet here there can be no nuclei formed of products of combustion, for there is no combustion; there is simply ignition or incandescence.

One other point I may mention. It is stated by Barus, in the paper above referred to, that the fumes given off by a piece of phosphorus constitute a most efficient cause of dense condensation. This is true if they come directly from a piece of phosphorus; but if phosphorus fumes are collected in a bottle and then directed upon the jet, all traces of unoxidised phosphorus being first carefully removed, they are found to be absolutely inoperative. Phosphorus in air can hardly be said to be incandescent, though it is luminous in the dark; but it appears to act in the same manner as if its temperature were high.

All these facts seem to indicate that the several causes mentioned, electrical, chemical, and thermal, confer upon the air in which they act some temporary property—certainly not due to mere inert dust—in virtue of which it acquires an abnormal power of promoting aqueous condensation.

I thought that possibly some clue as to the nature of this property might be obtained by observing how some other gases and vapours behaved; but though the experiments I made perhaps tend to narrow the dimensions of the mystery, I cannot say that they have completely solved it. Indeed some of the results only introduce additional perplexities.

One of the most natural things to try is hydrochloric acid, which is known to have a strong affinity for water. If we heat a little of the acid solution in a test tube, closed with a cork, through which a glass tube is passed, and direct the issuing stream of gas upon the jet, the densest condensation results. The vapours of sulphuric and nitric acids also cause dense condensation, and I suppose both of these have an affinity for water. But so also, and in an equally powerful degree, does the vapour of acetic acid; yet the affinity of this acid for water, as indicated by the heat evolved when the two are mixed, is very small.

Ammonia gas, when dissolved in water, causes the evolution of much heat. Yet a stream of this gas directed upon the jet has no action.

Ozonised air, which Mascart found so effective in his experiments with the closed vessel, is quite inoperative with the steam jet. Equally so is the vapour of boiling formic acid, which I believe is chemically a much more active acid than acetic, and has a lower electrical resistance. (See Table.)

CONDENSATION OF STEAM JET.

Active.

Air, oxygen, or nitrogen, in which electrical discharge is occurring.

Burning and incandescent substances.
Fumes from phosphorus.
Hydrochloric acid.
Sulphuric acid vapour.
Nitric acid vapour.
Acetic acid vapour.

Inactive.

Air, &c., in which electrical discharge has ceased for about ten seconds.

Smoke without fire.
Bottled phosphorus fumes.
Ammonia.
Ozone.
Steam.
Alcohol vapour.
Formic acid vapour.
Sulphurous acid.

It seems that we have here a pretty little problem which might, perhaps, be solved without much difficulty by a competent chemist, but which quite baffles me.[1] Is it possible that the condensing vapours may contain dissociated atoms?

To return to the electrical effect. There are only two kinds of chemical change that I know of which could be brought about in air by an electrical discharge. Either some of the oxygen might be converted into ozone, or the oxygen and nitrogen of the air might be caused to combine, forming nitric acid or some such compound. The former of these would not account for the action of the air upon the jet, because, as we have seen, ozone is inoperative; the latter might. But if the activity of the air is due to the presence in it of a compound of oxygen and nitrogen, then it is clear that an electrical discharge in either nitrogen or oxygen separately would fail to render those gases active.

I arranged a spark bottle, inside which an induction-coil discharge could be made to take place; two bent tubes were passed through the cork, one reaching nearly to the bottom for the ingress of the gas to be tested, the other, a shorter one, for its egress. The open end of the egress tube was fixed near the steam jet, and first common air, then oxygen and then nitrogen were successively forced through the bottle while the coil discharge was going on. All produced dense condensation, but I thought that oxygen appeared to be a little more efficient than common air, and nitrogen a little less.

This last experiment points to a conclusion to which at present I see no alternative. It is that the action on the jet of an electrical discharge is due in some way or other to dissociated atoms of oxygen and nitrogen. There is nothing else left to which it *can* be due.

So far as Robert Holmholtz's explanation coincides with this conclusion, I think it must be accepted as correct. As to the precise manner in which he supposed the dissociated atoms to act upon the jet, it is more difficult to agree with him. He thought that the abnormal condensation was a consequence of the molecular shock caused by the violent recombination of the dissociated atoms in the supersaturated air of the jet, the action being analogous to that which occurs when a supersaturated solution of sulphate of soda, for example, is instantly crystallised by a mechanical shock.

To me this hypothesis, ingenious as it is, seems to be more fanciful than probable, but I can only hint very diffidently at an alternative one. To many chemical processes the presence of water is favourable or even essential. Is it possible that the recombination of free atoms may be assisted by water? And is it possible that dissociated atoms in an atmosphere of aqueous vapour may obtain the water needed for their union by condensing it from the vapour?

According to Holmholtz, flames and incandescent substances generally cause dissociation of the molecules of oxygen and nitrogen in the surrounding air. This, I believe, is generally admitted. I do not know whether slowly oxidising phosphorus has the same effect.

If it is conceded that the atmospheric gases are dissociated by electrical discharges, and that the presence of such dissociated gases somehow brings about the dense condensation of water vapour, we may still regard the electrified steam jet as affording an illustration of the abnormal darkness of thunder-clouds.

Perhaps another source of dissociated atoms is to be found in the ozone which is generated by lightning flashes. A molecule of ozone consists of three atoms of atomic oxygen, while one of ordinary oxygen contains only two. Ozone is an unstable kind of material, and gradually relapses into ordinary oxygen, the process being that one atom is dropped from the three-atom molecules of ozone, these detached atoms in course of time uniting with one another to form pairs. Thus two molecules of ozone are transformed into three of oxygen. A body of ozone is therefore always attended by a number of dissociated atoms which are looking for partners.

In the steam jet experiment there is not time for the disengagement of a sufficient number of isolated atoms from a blast of ozone to produce any sensible effect. But the case is otherwise when the vapour is confined in a closed vessel, as in Mascart's experiment, or when it occurs in the clouds, where the movement of air and vapour is comparatively slow.

Ozone, it will be remembered, was found by Mascart to produce dense condensation in a closed vessel even after being filtered through cotton wool. Similar filtration seems to entirely deprive the so-called products of combustion of their active property, a fact which has been adduced as affording overwhelming evidence in favour of the dust nucleus theory. Coulier himself, however, detected a weak point in this argument. He produced a flame which could not possibly have contained any products of combustion except steam, by burning pure filtered hydrogen in filtered air; yet this product was found to be perfectly capable of causing dense condensation, and, as in his former experiments, filtration through cotton wool deprived it of its activity.

These anomalies may, I think, be to a great extent cleared up if we assume that the effect of the cotton wool depends, not upon the mere mechanical obstruction it offers to the passage of particles of matter, but upon the moisture which it certainly contains, and which may act by attracting and facilitating the reunion of dissociated atoms before they reach the air inside the vessel. According to this view ozone would remain an active condenser in spite of its filtration, because free atoms would continue to be given off by it after it had passed the cotton wool. The filtration experiment should be tried with perfectly dry cotton wool, which, however, will not be easily procured, and if my suggestion is right, dry wool will be found not to deprive ordinary products of combustion of their condensing power.

To sum up. I think my recent experiments show conclusively that the dense condensation of the steam jet is not due directly either to electrical action or to dust nuclei. The immediate cause is probably to be found in dissociated atoms of atmospheric gases, though as to how these act we can only form a vague guess.

SHELFORD BIDWELL.

[1] Two chemists of the highest eminence have been good enough to consider the problem for me, but they are unable to throw any light upon it.

SCIENTIFIC SERIALS.

American Journal of Science, December.—An apparent time-break between the eocene and Chattahoochee miocene in south-western Georgia, by Raphael Pumpelly. The Red Clay Hill region, a plateau extending through the south-western part of Georgia and adjacent northern Florida, has a maximum altitude of 300 feet, is sharply limited on the north by a declivity facing the eocene flat-land country, and consists of miocence deposits resting on eocene, both of which dip about 13 feet per mile to the south. The base of the plateau is formed by the white calcareous beds of the Chattahoochee group. A time-break between the latter and the eocene is evidenced by the almost general presence of a limestone conglomerate at the base of the Chattahoochee, immediately overlying eocene fossils, and the irregularity of the surface of demarcation. It seems possible that during miocene time the present plateau of southern Georgia was outlined by submerged islands of the eocene limestone. The Gulf Stream, after the creation of the central American barrier, found its way back to the Atlantic sweeping over southern Georgia and northern Florida, and supplying the food needed to build up the great organic beds of the Chattahoochee and Chipola. The lower flat-land country of central Georgia may represent the contemporaneous course of the cold current carrying less pure water and less nutriment.—The rise of the mammalia in North America, by H. F. Osborn. This second part deals with ancient and modern placental differentiation, the succession of the perissodactyls and the artiodactyls, a discussion of the factors of evolution, and a diagram illustrating the supposed descent of the mammalia from their jurassic prototypes.—On the thoracic legs of Triarthrus, by C. E. Beecher. Some very perfect specimens of *Triarthrus Becki*, Green, in which nearly the entire calcareous and chitinous portions are represented by a thin film of iron pyrites, show, besides the antennæ already noticed, a complete series of thor-

acic legs becoming shorter towards the pygridium, but without any essential differences amongst each other. Each limb consists of two nearly equal members, one of which was evidently used for crawling, and the other for swimming. These two members and their joints may be correlated with certain typical forms of Crustacean legs among the *Schizopoda*, *Cumacea*, and *Decapoda*, and may be described in the same terms.—On the diamond in the Cañon Diablo meteoric iron and on the hardness of carborundum, by George F. Kunz and Oliver W. Huntingdon. The carborundum made by Mr. Acheson, of Pittsburg, is capable of scratching most varieties of corundum, but not the diamond.

SOCIETIES AND ACADEMIES.

LONDON.

Anthropological Institute, December 12.—Prof. A. Macalister, F.R.S., President, in the chair.—Mr. Cuthbert E. Peek exhibited some specimens of fishing-line made of human hair, some needles constructed from ribs of feather, and two message sticks from the extreme north of Queensland.—Mr. W. L. Duckworth read a paper on the collection of skulls of Aboriginal Australians in the Cambridge University Museum, and the following papers were also read:—On an unusual form of rush basket from the northern territory of South Australia, by Mr. R. Etheridge, jun.—On a modification of the Australian Aboriginal weapon, termed the leonile, langeel, bendi or buccan, by Mr. R. Etheridge, jun.—An Australian Aboriginal musical instrument, by Mr. R. Etheridge, jun.—The Aborigines of North-West Australia, by Mr. P. W. Bassett-Smith.—Rites and customs of Australian Aborigines, by Mr. H. B. Purcell.—Japanese onomatopes and the origin of language, by Mr. W. G. Aston.

Mathematical Society, December 14.—A. B. Kempe, F.R.S., President, in the chair.—On the stability of a deformed elastic wire, by A. B. Basset, F.R.S.—This paper commences with a discussion of the different methods of determining the stability of a deformed elastic wire which is in equilibrium, and then proceeds to discuss two special problems. When a naturally straight wire is deformed into a helix having m convolutions, the helical form is unstable unless its pitch is greater than $\sec^{-1} 2m$. This result shows that it is impossible to deform the wire into a helix of *small* pitch and having a great many convolutions, such as the spring of an ordinary spring-balance, unless the wire is given a permanent set. The two special cases in which the terminal stresses consist, (1) of a thrust and a flexural couple, (2) of a couple alone, are also noticed; and in the latter case the helix is unstable when the length of the wire exceeds half a convolution. When the natural form of the wire is a circular coil, which is unrolled and the ends joined together without twist, so that the wire forms a circular ring, the ring will be unstable when the length of the wire is greater than about one and a half convolutions. The ring is stable from displacements in its plane, and consequently will not collapse like a boiler flue; but it is unstable for displacements perpendicular to its plane, which involve torsion as well as flexion. The stable figure will consequently consist of a closed tortuous curve.—Papers were also read by R. J. Dallas, on the linear automorphic transformations of certain quantics; and by Dr. Hobson, F.R.S., on Bessel's functions and relations connecting them with spherical and hyperspherical harmonics.—Messrs. Love, Greenhill, Macmahon, and the President spoke on the subject of the communications.—The following papers were taken as read:—A theorem of Liouville's, by Prof. G. B. Mathews; note on non Euclidian geometry, by H. F. Baker; note on an identity in elliptic functions, by Prof. L. J. Rogers; and note on a variable seven-points circle analogous to the Brocard circle of a plane triangle, by J. Griffiths.

Royal Meteorological Society, December 20.—Dr. C. Theodore Williams, President, in the chair.—Mr. C. Harding gave an account of the great storm of November 16 to 20, 1893. This storm was the most violent of recent years, and, so far as anemometrical records are concerned, the wind attained a greater velocity than has previously been recorded in the British Islands. The velocity of the wind was 96 miles in the hour from 8.30 to 9.30 p.m. on November 16 in the Orkneys, where the hurricane burst with such suddenness that it is described as like the shot of a gun, and the wind afterwards attained the very high rate of 90 miles and upwards, in the hour, for 5 consecutive hours. At Holyhead the storm was terrific; the anemometer recorded a wind velocity of 89 miles in the hour, and it was 80 miles or above for 11 hours, while the force of a whole gale, 65 miles an hour and upwards, was maintained for 31 hours, and for 4½ days the mean hourly velocity was 54 miles. Many of the gusts were at the rate of 115 miles an hour, and at Fleetwood a squall occurred with the wind at the rate of 120 miles in the hour. The storm was felt over the entire area of the United Kingdom, and the wreck returns show that disasters occurred with almost equal frequency on all coasts. Four weeks after the storm the official records gave the total loss of life on our coasts as 335, while there were 140 vessels which had been abandoned, or had foundered, stranded, or met with other severe casualty, involving either loss of life, or saving of life by some extraneous assistance. There were 600 lives saved on our coasts by aid of the Lifeboat Institution and other means. The author has tracked the storm from the neighbourhood of the Bahamas on November 7, acress the Atlantic and over the British Islands to Central Europe on November 20.—The other papers read were on rainfull and evaporation observations at the Bombay Waterworks, by Mr. S. Tomlinson; and on changes in the character of certain months, by Mr. A. E. Watson.

DUBLIN.

Royal Dublin Society, November 22.—Prof. W. N. Hartley, F.R.S., in the chair.—Prof. T. Johnson communicated a paper on the systematic position of the Bangiaceæ. The author, with Berthold and others, regards the group as true Florideæ, and discusses in his paper the views expressed by Schmitz, in a recent number of *La Nuova Notarisia*, against their Floridean nature—Mr. Thomas Preston gave an elementary explanation of the system of waves attending a bullet moving at a high speed through the atmosphere.—Mr. W. E. Adeney read a note on the present condition of the water in the Vartry reservoir at Roundwood, co. Wicklow, and Mr. Richard J. Moss gave the results of an examination of the Vartry water as at present supplied to Dublin.

PARIS.

Academy of Sciences, Annual Public Meeting, December 18.—M. de Lacaze-Duthiers in the chair.—After some commemorative words on the deaths of Sir Richard Owen, Kummer, and de Candolle, Foreign Associates, and those of Chambrelent, Admiral Pâris and Charcot, Members of the Academy, by the President, M. Bertrand, one of the Secretaries, announced the names of those to whom prizes had been awarded. In *Geometry*, the Prix Francœur was awarded to M. G. Robin for mathematical physics, and the Prix Poncelet to M. G. Kœnigs, for geometrical and mechanical work.—*Mechanics:* The extraordinary prize of 6000 francs offered by the Département de la Marine for contrivances increasing the efficiency of the Navy, was distributed among M. Bourdelles (for lighthouse illumination), M. Lephay (compass with luminous index), and M. de Fraysseix (system of optical pointing); the Prix Montyon of 700 francs to M. Flamant (hydraulics), the Prix Plumey of 2500 francs to M. Lebasteur (steam engine appliances); the Prix Fourneyron of 500 francs, to M. Brousset (fly-wheels).—*Astronomy:* The Prix Lalande of 540 francs, to M. Schulhof (Comets); the Prix Valz of 460 francs, to N. Berberich (Minor Planets). The Prix Janssen of a gold medal, to Mr. Samuel Langley (Astronomical Physics).—*Physics:* The Prix La Caze of 10,000 fr., to M. E. H. Amagat (gases and liquids).—*Statistics:* The Prix Montyon of 500 fr., to Dr. Marvand (diseases of soldiers).—*Chemistry:* The Prix Jecker of 10,000 fr., to M. D. Forcrand and M. Griner in equal parts, with a special prize to M. Gautier.—The Prix La Caze of 10,000 fr., to M. Lemoine (Phosphorus Compounds).—*Mineralogie and Geology:* The Grand Prix, to M. Marcellin Boule (The Central Plateau of France). The Prix Bordin of 3000 fr. was distributed amongst MM. Bourgeois, Gorgen, Michel, and Duboin for their researches in mineral synthesis. The Prix Delesse of 1400 fr., to M. Fayol (Commentry Strata). The Prix Fontannes of 2000 fr., to M. R. Zeiller (Palæontology).—*Botany:* The Prix Desmazières of 1600 fr., to M. C. Sauvageau (Algæ). The Prix Montagne, to MM. Cardot (Mosses) and Gaillard (Fungi).—*Agriculture:* The Prix

Morogues, to M. Millardet (Mildew).—*Anatomy and Zoology.* The Prix Thore, to M. Corbière (Muscineæ).—*Medicine and Surgery:* The Prix Montyon was distributed between MM. Huchard (Heart Diseases), Delorme (Army Surgery), and Pinard and Varnier (Pathological Atlas). The Prix Barbier, 500 fr. each to MM. Sanson (Heredity) and Dr. Mauclaire (Osteo-Arthritis). The Prix Bréant, being the interest on a sum of 100,000 francs offered for a cure for cholera, was distributed amongst MM. Netter and Thoinot (French Cholera, 1892) and MM. Grimbert and Burlureaux (Treatment of Tuberculosis by Creosote Injections). The Prix Godard of 1,000 francs, to Dr. Tourneux (Physiological Atlas). The Prix Serres of 7500 francs, to M. Pizon (Blastogenesis), with small portions to MM. Sabatier (Spermatogenesis) and Letulle (Inflammation). The Prix Bellion of 1400 francs, to Dr. C. Chabrié (Physiology of the Kidney) and Dr. Coustan (Fatigue). The Prix Mège to Dr. Herrgott (History of Obstetrics). The Prix Lallemand of 1800 francs, to M. Trolard (Venous System).—*Physiology:* The Prix Montyon of 750 francs, to M. Laulanié (Respiration) and MM. Abelous and Langlois (Renal Capsules). The Prix La Caze, of 10,000 francs, to M. d'Arsonval (Physiological Effects of Electricity). The Prix Pourat to M. E. Meyer (Renal Secretion). The Prix Martin-Damourette, of 1400 francs, to Dr. Géraud (Albuminuria).—*General Prizes:* The Arago Medal to Mr. Asaph Hall (Satellites of Mars) and Mr. E. E. Barnard (Jupiter's First Satellite). The Prix Montyon, for improvements in unhealthy industries, was divided between MM. Garros (Porcelain Manufacture) and Coquillon (Fire-damp Meter). The Prix Trémont, of 1100 francs, to M. Jules Morin for his useful hydrostatic and other inventions. The Prix Gegner of 4000 francs to M. Serret. The Prix Petit d'Ormoz of 10,000 francs, to M. Stieltjes (Mathematics), and another of the same amount to M. Marcel Bertrand (Physics of the Globe). The Prix Tchihatchef of 10,000 francs, to M. Grégoire Groum-Grschimailo (The Pamirs). The Prix Gaston Planté, of 3,000 francs, to M. Blondlot (Electric Interference). Mme. de Laplace's Prize, consisting of Laplace's works, to M. Bès de Berc, of the École Nationale des Mines.

BERLIN.

Physical Society, December 1.—Prof. Schwalbe, President, in the chair.—Prof. Neesen demonstrated a method of coating aluminium with other metals. This consists in dipping the aluminium in a solution of caustic potash or soda, or of hydrochloric acid, until bubbles of gas make their appearance on its surface, whereupon it is dipped into a solution of corrosive sublimate to amalgamate its surface. After a second dipping into caustic potash until bubbles of gas are evolved, the metal is placed in a solution of a salt of the desired metal. A film of the latter is rapidly formed, and is so firmly adherent that, in the case of silver, gold, or copper, the plate can be rolled out or polished. When coating with gold or copper, it is well to first apply a layer of silver. When thus treated the aluminium may be soldered with ordinary zinc solder.—Dr. Wien spoke on the entropy of radiation.

Meteorological Society, November 7.—Prof. von Bezold, President, in the chair.—Dr. Arendt spoke on the transport of heat by means of aerial currents on the earth's surface, based on calculations derived from material provided by the Hamburg station. He first determined for each month of the year the direction and rate of the wind, from which he calculated the resultant volume of air transported over Hamburg. From the temperature and speed of the winds he obtained, under certain assumptions, numerical values for the amount of heat carried towards Hamburg during each month of the year.

December 5.—Dr. Vettin, President, in the chair.—Prof. Hellmann presented a book on "Snow-crystals," and gave an account of its contents, during which he discussed fully the structure and classification of snow-crystals. All the crystals belong to the hexagonal system, and are either flat or columnar. The radiating stars, the plates, and mixed forms belong to the first category; while the prisms and much more rare pyramids belong to the second.—Dr. H. Meyer communicated the results of his observations, made in conjunction with Prof. Köppen, on the cloud-conditions of various climates. They had rejected as valueless mean values based on determinations which are largely influenced by the personal opinion of the observer, and had in preference calculated the frequency of the occurrence of clouds. They had in this, for simplicity's sake, distinguished between three groups: (1) Complete absence of clouds; cloudiness zero. (2) Intermittent occurrence of clouds; cloudiness 1 to 9. (3) Total cloudiness represented by 10. Taking a series of stations in various climates, they had calculated and graphically represented the frequency of the three groups for the morning, midday, and evening for each month. It appeared that for Hamburg and the whole of middle and north Europe, in passing from the cold to the warm periods of both the day and year, the intermittent cloudiness increases; while complete cloudiness, which is most frequent in winter, and in the morning and evening, diminishes. Complete cloudlessness is always the most rare condition. The above characters change gradually towards the Mediterranean, even at Lesina, and more markedly at Alexandria. In mid-Asia, East Siberia, China, Batavia, and Rio Janeiro, and on the elevated station of Pike's Peak, and also on the Atlantic Ocean, the change in cloudiness in passing from winter to summer is reversed.

BOOKS and SERIALS RECEIVED.

BOOKS.—A Text-Book on Gas, Oil, and Air-Engines: B. Donkin, Jun. (Griffin).—An Elementary Treatise on Fourier's Series: Dr. W. E. Byerly (Boston, Ginn).—Uniplanar Algebra: Dr. J. Stringham (San Francisco, Berkeley Press).—Science and Hebrew Tradition: T. H. Huxley (Macmillan).—Dictionary of the Active Principles of Plants: C. E. Sohn (Baillière).—The Country and Church of the Cheeryble Brothers: Rev. W. H. Elliot (Selkirk, Lewis).—Hints to Travellers, 7th edition (Royal Geographical Society).—The Story of the Sun: Sir R. Ball (Cassell).

SERIALS.—Insect Life, Vol. 6, No. 2 (Washington).—Cabinet Portrait Gallery, Part 52 (Cassell).—Astronomy and Astro-Physics, December (Wesley).—Economic Journal, December (Macmillan).—Journal of the Franklin Institute, December (Philadelphia).—Internationales Archiv für Ethnographie, Band vi. Heft 6 (Kegan Paul).—Journal of the Royal Microscopical Society, December (Williams and Norgate).—Royal Geographical Society, Supplementary Papers, Vol. III. Part 5 (Murray).

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